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IS THERE A LOGIC OF SCIENTIFIC DISCOVERY?

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The approved answer to this is "No". Thus Popper argues (*The Logic of Scientific Discovery*) "The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it." (p. 31.) Again, ". . . there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process." (p. 32.) Reichenbach writes that philosophy of science ". . . cannot be concerned with [reasons for suggesting hypotheses], but only with [reasons for accepting hypotheses]." (*Experience and Prediction,* p. 382.) Braithwaite elaborates: "The solution of these historical problems involves the individual psychology of thinking and the sociology of thought. None of these questions are our business here." (*Scientific Explanation,* pp. 20, 21.)

Against this negative chorus, the 'Ayes' have not had it. Aristotle (*Prior Analytics,* II, 25), and Peirce (*Collected Papers,* I, Sec. 188) hinted that in science there may be more problems for the logician than just analyzing the arguments supporting already-invented hypotheses. But contemporary philosophers are unreceptive to this. Let us try once again to discuss the distinction F. C. S. Schiller made between the 'Logic of Proof' and the 'Logic of Discovery'. (Cf. *Studies in the History and the Methods of the Sciences,* ed. Charles Singer.) We may be forced, with the majority, to conclude 'Nay.' But only after giving Aristotle and Peirce a sympathetic hearing. Is there *anything* in the idea of a 'logic of discovery' which merits the attention of a tough-minded, analytic logician?

It is unclear what a logic of discovery is a logic of. Schiller intended nothing more than "a logic of inductive inference". Doubtless his colleagues were so busy sectioning syllogisms, that they ignored inferences which mattered in science. All the attention philosophers now give to inductive reasoning, probability, and the principles of theory-construction, would have pleased Schiller. But, for Peirce, the work of Popper, Reichenbach and Braithwaite would read less like a *Logic of Discovery* than like a *Logic of the Finished Research Report.* Contemporary logicians of science have described how one sets out reasons in support of an hypothesis once proposed. They have said nothing about the conceptual context within which such an hypothesis is initially proposed. Both Aristotle and Peirce insisted that the proposal of an hypothesis can be a reasonable affair. One can have good reasons, or bad, for suggesting one kind of hypothesis initially, rather than some other kind. These reasons may differ in type from those which lead one to accept an hypothesis once suggested. (This is not to deny that one's reasons for proposing an hypothesis initially may be identical with his reasons for later accepting it.)

One thing must be stressed. When Popper, Reichenbach, and Braithwaite urge that there is no logical analysis appropriate to the psychological complex which attends the conceiving of a new idea, they are saying nothing which Aristotle or Peirce would reject. The latter did not think themselves to be writing manuals to help scientists make discoveries. There could be no such manual. ("There is no science which will enable a man to bethink himself of that which will suit his purpose", J. S. Mill, *A System of Logic,* III, Chapter I.) Apparently they felt that there is a *conceptual* inquiry, one properly called "a logic of discovery", which is *not* to be confounded with the psychology and sociology appropriate to understanding how some investigator stumbled on to an improbable idea in unusual circumstances. There are factual discussions such as these latter. Historians like Sarton and Clagett have undertaken such circumstantial inquiries. Others, e.g., Hadamard and Poincaré, have dealt with the psychology of discovery. But these are not logical discussions. They do not even turn on conceptual distinctions. Aristotle and Peirce thought they were doing something other than psychology, sociology, or history of discovery; they purported to be concerned with a *logic* of discovery.

This suggests caution for those who reject wholesale any notion of a logic of discovery on the grounds that such an inquiry can *only* be psychology, sociology, or history. That Aristotle and Peirce deny just this has made no impression. Perhaps Aristotle and Peirce were wrong. Perhaps there is no room for logic between the psychological dawning of a discovery and the justification of that discovery *via* successful predictions. But this should come as the conclusion of a discussion, not as its preamble. If Peirce is correct, nothing written by Popper, Reichenbach or Braithwaite cuts against him. Indeed, these authors do not discuss what Peirce wishes to discuss.

Let us begin this uphill argument by distinguishing

(1) reasons for accepting an hypothesis H, from

(2) reasons for suggesting H in the first place.

This distinction is in the spirit of Peirce's thesis. Despite his arguments, most philosophers deny any *logical* difference between these two. This must be faced. But let us shape the distinction before denting it with criticism.

What would be our reasons for accepting H? These will be those we might have for thinking H true. But the reasons for suggesting H originally, or for formulating H in one way rather than another, may not be those one requires before thinking H true. They are, rather, those reasons which make H a *plausible type of conjecture.* Now, no one will deny *some* differences between what is required to show H true, and what is required for deciding that H constitutes a plausible kind of conjecture. The question is: are these logical in nature, or more properly called "psychological" or "sociological"?

Or, one might urge (as does Professor Feigl) that the difference is just one of refinement, degree, and intensity. Feigl argues that considerations which settle whether H constitutes a plausible conjecture are of the *same type* as those which settle whether H is true. But since the initial proposal of an hypothesis is a groping affair, involving guesswork amongst sparse data, there *is* a distinction to be drawn; but this, Feigl urges, concerns two ends of a spectrum ranging all the way from inadequate and badly selected data, to that which is abundant, well-diversified, and buttressed by a battery of established theories. The issue therefore remains: is the difference between reasons for accepting H and reasons for suggesting it originally, one of logical type, or one of degree, or of psychology, or of sociology?

Already a refinement is necessary if our original distinction is to survive. The distinction just drawn must be re-set in the following, more guarded, language. Distinguish now

(1′) reasons for accepting a particular, minutely-specified hypothesis H, from

(2′) reasons for suggesting that, whatever specific claim the successful H will make, it will nonetheless be an hypothesis of one *kind* rather than another.

Neither Aristotle, nor Peirce, nor (if you will excuse the conjunction) myself in earlier papers, sought this distinction on these grounds. The earlier notion was that it was some particular, minutely-specified H which was being looked at in two ways: (1)

what would count for the acceptance of that H, and (2) what would count in favour of suggesting that same H initially.

This way of putting it is objectionable. The issue is, whether (*before* having hit on an hypothesis which succeeds in its predictions) one can have good reasons for anticipating that the hypothesis will be one of some particular *kind*. Could Kepler, e.g., have had good reasons (*before* his elliptical orbit hypothesis was established) for supposing that the successful hypothesis concerning Mars' orbit would be of the non-circular kind?[1] He *could* have argued that, whatever path the planet *did* describe, it would be a closed, smoothly-curving, plane geometrical figure. Only this *kind* of hypothesis could entail such observation statements as that Mars' apparent velocities at 90° and at 270° (of excentric anomaly) were greater than any circular-type H could explain. Other *kinds* of hypotheses were available to Kepler: e.g., that Mars' *colour* is responsible for its high velocities, or that the dispositions of Jupiter's moons are responsible. But these would not have struck Kepler as capable of explaining such surprising phenomena. Indeed, he would have thought it *un*reasonable to develop such hypotheses at all, and would have argued thus. (Braithwaite counters: "But exactly which hypothesis was to be rejected was a matter for the 'hunch' of the physicists" (*Scientific Explanation,* p. 20). However, which *type* of hypothesis Kepler chose to reject was not just a matter of 'hunch'.)

I may still be challenged. Some will continue to berate my distinction between reasons for suggesting which type of hypothesis H will be, and reasons for accepting H ultimately.[2] There may indeed be "psychological" factors, the opposition concedes, which make certain types of hypothesis 'look' as if they might explain phenomena. Ptolemy knew, as well as did Aristarchus before him and Copernicus after him, that a kind of astronomy which displaced the earth would be theoretically simpler, and easier to manage, than the hypothesis of a geocentric, geostatic universe. *But,* philosophers challenge, for psychological, sociological, or historical reasons, alternatives to geocentricism did not 'look' as if they could explain the absence of stellar parallax. This cannot be a matter of logic, since for Copernicus one such alternative *did* 'look' as if it could explain this. In so far as scientists have *reasons* for formulating types of hypotheses (as opposed to hunches, and intuitions), these are just the kinds of reasons which later show a particular H to be true. Thus, if the absence of stellar parallax constitutes more than a psychological reason for Ptolemy's resistance to alternatives to geocentricism, then, in so far, it *is* his reason for rejecting such alternatives as *false*. Conversely, his reason for developing a geostatic type of hypothesis (again, absence of parallax) was his reason for taking some such hypothesis as *true*. Again, Kepler's reasons for rejecting Mars' colour or Jupiter's moons as indicating the kinds of hypotheses responsible for Mars' accelerations, were reasons which also served later in establishing some hypothesis of the non-circularity type.

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[1] Cf. *De Motibus Stellae Martis* (Munich, pp. 250 ff).

[2] Reichenbach writes that philosophy "cannot be concerned with the first, but only with the latter" (*Experience and Prediction,* p. 382).

So the objection to my distinction is: the only *logical* reason for proposing that H will be of a certain type is that *data* incline us to think some *particular* H true. What Hanson advocates is psychological, sociological, or historical in nature; it has no logical import for the differences between proposing and establishing hypotheses.

Kepler again illustrates the objection. Every historian of science knows how the idea of uniform circular motion affected astronomers before 1600. Indeed, in 1591 Kepler abandons an hypothesis because it entails other-than-uniform circular orbits—simply inconceivable for him. So psychological pressure against forming alternative types of hypothesis was great. But *logically* Kepler's reasons for entertaining a type of Martian motion other than uniformly circular *were* his reasons for accepting that as astronomical truth. He first encountered this type of hypothesis on perceiving that no simple adjustment of epicycle, deferent, and excentric could square Mars' observed distances, velocities, and apsidal positions. These were also reasons which led him to assert that the planet's orbit is not the effect of circular motions, but of an elliptical path. Even after other inductive reasons confirmed the truth of the latter hypothesis, these early reasons were *still* reasons for accepting H as true. So they cannot have been reasons merely for proposing which type of hypothesis H would be, and nothing more.

This objection has been made strong. If the following cannot weaken it, then we shall have to accept it; we shall have to grant that there is *no* aspect of discovery which has to do with logical or conceptual considerations.

When Kepler published *De Motibus Stellae Martis* he had established that Mars' orbit was an ellipse, inclined to the ecliptic, the sun in one of the foci. Later (in the *Harmonices Mundi*) he generalized this for other planets. Consider the hypothesis H′: *Jupiter's* orbit is of the non-circular type.

The reasons which led Kepler to formulate H′ were many. But they included this: that H (the hypothesis that *Mars*' orbit is elliptical) is true. Since Eudoxos, Mars had been the typical planet. (*We* know why. Mars' retrogradations and its movement around the empty focus—all this Earth observes with clarity because of our spatial relations with Mars.) Now, Mars' dynamical properties are usually found in the other planets. If its orbit is ellipsoidal, then it is reasonable to expect that, whatever the exact shape of the other orbits (e.g., Jupiter's) they will all be of the non-circular type.

But such reasons would not *establish* H′. Because what makes it reasonable to anticipate that H′ will be of a certain type is *analogical* in character. (Mars does x; Mars is a typical planet; so, perhaps, all planets do the same kind of thing as x.) Analogies cannot establish hypotheses, not even *kinds* of hypotheses. Only observations can do that; in this the Hypothetico-Deductive account (of Popper, Reichenbach and Braithwaite) is correct. To establish H′ requires plotting its successive positions on a smooth curve whose equations can be determined. It may then be possible to assert that Jupiter's orbit is, e.g., an ellipse, an oviform, an epicycloid, or whatever. But it would not be reasonable to expect this when discussing only what type of hypothesis is likely to describe Jupiter's orbit. Nor is it right to characterize this difference between H-as-illustrative-of-a-type-of-hypothesis, and H-as-empirically-established, as a difference of psychology only. *Logically,* Kepler's analogical reasons for proposing that H′ would be of a certain type were good reasons. But, logically, they would not then have been good reasons for asserting the truth of a specific value for H′—something which could be done only years later.

What are and are not good reasons for reaching a certain conclusion is a logical matter. No further observations are required to settle such issues, any more than we require experiments to decide, on the basis of one's bank statements, whether one is bankrupt. Similarly, whether or not Kepler's reasons for anticipating that H′ will be of a certain kind are good reasons, this is a logical inquiry.

Thus, the differences between reasons for expecting that some as-yet-undiscovered H will be of a certain type, and those which establish that H, are greater than is conveyed by calling them "psychological", "sociological", or "historical".

Kepler reasoned initially by analogy. Another kind of reason which makes it plausible to propose that an H, once discovered, will be of a certain type, could be the detection of a formal symmetry in sets of equations or arguments. At important junctures Clerk Maxwell and Einstein detected such structural symmetries.

This allowed them to argue, before getting their final answers, that those answers would be of a clearly describable type.

In the late 1920's, before anyone had explained the "negative-energy" solutions in Dirac's electron theory, good analogical reasons could have been advanced for the claim that, whatever specific assertion the ultimate H assumed, it would be of the Lorentz-invariant type. It could have been conjectured that the as-yet-undiscovered H would be compatible with the Dirac explanation of Compton scattering and doublet atoms, and would fail to confirm Schrödinger's hunch that the phase waves within configuration space actually described observable physical phenomena. All this could have been said before Weyl, Oppenheimer, and Dirac formulated the "Hole-theory of the positive electron". Good analogical reasons for supposing that the *type* of H which would succeed would be along these lines could have been, and were, advanced. Indeed, Schrödinger's attempt to rewrite the Dirac theory so that the negative-energy solutions disappeared was *rejected* for failing to preserve Lorentz-invariance.

Thus, reasoning from observations of As as Bs to the proposal "all As are Bs" is different in type from reasoning analogically from the fact that Cs are Ds to the proposal "the hypothesis relating As and Bs will be the same type as that relating Cs and Ds". (Here it is the *way* Cs are Ds which seems analogous to the way As are Bs.) And both of these are typically different from reasoning involving the detection of symmetries in equations describing As and Bs.

Indeed, put in this way, what could an objection to the foregoing consist in? Establishing an hypothesis, and proposing by analogy that an hypothesis is likely to be of a particular type: surely these follow reasoning which is different in type. Moreover, both procedures have a fundamentally logical or conceptual interest.

An objection: "Analogical arguments, and those based on the recognition of formal symmetries, are used because of inductively established beliefs in the reliability of arguments of that type. So the cash value of such appeals ultimately collapses into just those accounts given by H-D theorists."

Agreed. But we are not discussing the *genesis* of our faith in these types of arguments, only the *logic* of the arguments themselves. *Given* an analogical premise, or one based on symmetry considerations—or even on enumeration of particulars—one argues *from* these in logically different ways. Consider what further moves are necessary to convince one who doubted such arguments. A challenge to "All As are Bs", when this is based on induction by enumeration, could only be a challenge to justify induction, or at least to show that the particulars are being correctly described. This is inappropriate when the arguments rest on analogies, or on the recognition of formal symmetries.

Another objection: "Analogical reasons, and those based on symmetry—these are *still* reasons for H even after it is (inductively) established. They are reasons *both* for proposing that H will be of a certain type, and for accepting H."

Agreed, again. But, analogical and symmetry arguments could never *by themselves* establish particular Hs. They can only make it plausible to suggest that H (when discovered) will be of a certain type. However, inductive arguments can, by themselves, establish particular hypotheses. So they must differ from arguments of the analogical or symmetrical sort.

H-D philosophers have been most articulate on these matters. So, let us draw out a related issue on which Popper, Reichenbach and Braithwaite seem to me not to have said the last word.

J. S. Mill was wrong about Kepler (*A System of Logic,* III, 2-3). It is impossible to reconcile the delicate adjustment between theory, hypothesis, and observation recorded in *De Motibus Stellae Martis* with Mill's statement that Kepler's First Law is but "a compendious expression for the one set of directly observed facts". Mill did not understand Kepler. (As Peirce notes, *Collected Papers,* I, p. 31.) It is equally questionable whether Reichenbach understood him: "Kepler's laws of the elliptic motion of celestial bodies were inductive generalizations of observed fact . . . [he] observed a series of . . . positions of the planets Mars and found that they may be connected by a mathematical relation . . ." (*Experience and Prediction,* p. 371). Mill's *Logic* is as misleading about scientific discovery as any account proceeding *via* what Bacon calls "*inductio per enumerationem simplicem ubi non reperitur instantia contradictoria*". (Indeed Reichenbach observes: "It is the great merit of John Stuart Mill to have pointed out that all empirical inferences are reducible to the *inductio per enumerationem simplicem* . . ." (*op. cit.,* p. 389).) The accounts of H-D theorists are equally misleading.

An H-D account of Kepler's First Law would treat it as a high-level hypothesis in an H-D system. (This is Braithwaite's language.) It is regarded as a quasi-axiom, from whose assumption observation-statements follow. If these are true—if, e.g., they imply that Uranus' orbit is an ellipse and that its apparent velocity at 90° is greater than at aphelion—then in so far is the First Law confirmed. (Thus Braithwaite writes: "A scientific system consists of a set of hypotheses which form a deductive system . . .

arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow . . . the establishment of a system as a set of true propositions depends upon the establishment of its lowest level hypotheses . . ." (*Scientific Explanation,* pp. 12-13).)

This describes physical theory more adequately than did pre-Baconian accounts in terms of simple enumeration, or even post-Millian accounts in terms of ostensibly not-so-simple enumerations. It tells us about the logic of laws, and what they do in finished arguments and explanations. H-D accounts do not, however, tell us anything about the context in which laws are proposed in the first place; nor, perhaps, were they even intended to.

The induction-by-enumeration story *did* intend to do this. *It* sought to describe good reasons for initially proposing H. The H-D account must be silent on this point. Indeed, the two accounts are not strict alternatives. (As Braithwaite suggests they are when he remarks of a certain higher-level hypothesis that it "will not have been established by induction by simple enumeration; it will have been obtained by the hypothetico-deductive method . . ." (*op. cit.,* p. 303).) They are thoroughly compatible. Acceptance of the second is no reason for rejecting the first. A law *might* have been inferred from just an enumeration of particulars (e.g., Boyle's law in the 17th century, Bode's in the 18th, the laws of Ampère and Faraday in the 19th, and much of Meson theory now). It could *then* be built into an H-D system as a higher order proposition. If there is anything wrong with the older view, H-D accounts do not reveal this.

There *is* something wrong. It is false. Scientists do not always discover every feature of a law by enumerating and summarizing observables. (Thus even Braithwaite says: "Sophisticated generalizations (such as that about the proton-electron constitution of the hydrogen atom) . . . [were] certainly not derived by simple enumeration of instances . . ." (*op. cit.,* p. 11).) But *this* does not strengthen the H-D account as against the inductive view. There is *no* H-D account of how "sophisticated generalizations" are *derived.* On his own principles, the H-D theorist's lips are sealed on this matter. But there are conceptual considerations which help us understand the *reasoning* which is sometimes successful in determining the type of an as-yet-undiscovered hypothesis.

Were the H-D account construed as a description of scientific practice, it would be misleading. (Braithwaite's use of "derived" is thus misleading. So is his announcement (p. 11) that he is going to explain "how we *come to make* use of sophisticated generalizations".) Natural scientists do not "start from" hypotheses. They start from data. And even then not from commonplace data—but from surprising anomalies. Thus Aristotle remarks (*Metaphysics* 982 b 11 ff.) that knowledge begins in astonishment. Peirce makes perplexity the trigger of scientific inquiry. (*Collected Papers,* II, Book III, ch. 2, Part III.) And James and Dewey treat intelligence as the result of mastering problem situations. (Dewey, *How We Think,* pp. 12 ff.)

By the time a law gets fixed into an H-D system, the *original* scientific thinking is over. The pedestrian process of deducing observation-statements begins only after the physicist is convinced that the proposed hypothesis is at least of the right type to explain the initially perplexing data. Kepler's assistant could work out the consequences of H′, and check its validity by seeing whether Jupiter behaved as H′ predicts. This was possible because of Kepler's argument that what H had done for Mars, H′ might do for Jupiter. The H-D account is helpful here; it analyzes *the argument of a completed research report.* It helps us see how experimentalists elaborate a theoretician's hypotheses. And the H-D account illuminates yet another aspect of science, but its proponents have not stressed it. Scientists often dismiss explanations alternative to that which has won their provisional assent along lines that typify the H-D method. Examples are in Ptolemy's *Almagest,* when (on observational grounds) he rules out a moving earth; in Copernicus' *De Revolutionibus . . .*, when he rejects Ptolemy's lunar theory; in Kepler's *De Motibus Stellae Martis,* when he denies that the planes of the planetary orbits intersect in the centre of the ecliptic; and in Newton's *Principia,* when he discounts the idea that the gravitational force law might be of an inverse cube nature. These mirror formal parts of Mill's *System of Logic* or Braithwaite's *Scientific Explanation.*

Still, the H-D analysis remains silent on reasoning which often conditions the discovery of laws—reasoning which determines which type of hypothesis it is likely to be most fruitful to propose.

The induction-by-enumeration story views scientific inference as being from observations to the law, from particulars to the general. There is something true about this which the H-D account must ignore. Thus Newton wrote: "the main business of natural philosophy is to argue from phenomena. . . ." (*Principia,* Preface.)

This inductive view ignores what Newton never did: the inference is also from *explicanda* to an *explicans.* Why a bevelled mirror shows spectra in sunlight is not explained by saying that all bevelled mirrors do this. Why Mars moves more rapidly at 270° and 90° than could be expected of circular-uniform motions is not explained by saying that Mars (or even all planets) always move thus. On the induction view, these latter might count as laws. But only when it is explained why bevelled mirrors show spectra and why planets apparently accelerate at 90° will we have laws of the type suggested: Newton's Laws of Refraction and Kepler's First Law. And even before such discoveries were made, arguments in favour of those *types* of law were possible.

So the inductive view rightly suggests that laws are somehow related to inferences *from* data. It wrongly suggests that the resultant law is but a summary of these data, instead of being an explanation of these data. A logic of discovery, then, might consider the structure of arguments in favour of one *type* of possible explanation in a given context as opposed to other *types*.

H-D accounts all agree that laws explain data. (Thus Braithwaite says: "A hypothesis to be regarded as a natural law must be a general proposition which can be thought to explain its instances; if the reason for believing the general proposition is solely direct knowledge of the truth of its instances, it will be felt to be a poor sort of explanation of these instances . . ." (*op. cit.,* p. 302).) However, they obscure the initial connection between thinking about data and thinking about what kind of hypothesis will be most likely to lead to a law. They suggest that the fundamental inference in science is from higher-order hypotheses to observation-statements. This may characterize the setting out of one's reasons for making a prediction after H is formulated and provisionally established. It need not be a way of setting out reasons in favour of proposing originally what type H is likely to be.

Yet the original suggestion of an hypothesis-type is often a reasonable affair. It is not as dependent on intuition, hunches, and other imponderables as historians and philosophers suppose when they make it the province of genius but not of logic. If the establishment of H through its predictions has a logic, so has the initial suggestion that H is likely to be of one kind rather than another. To form the first specific idea of an elliptical planetary orbit, or of constant acceleration, or of universal gravitational attraction does indeed require genius: nothing less than a Kepler, a Galileo, or a Newton. But this does not entail that reflections leading to these ideas are non-rational. Perhaps only Kepler, Galileo, and Newton had intellects mighty enough to fashion these notions initially; to concede this is not to concede that their reasons for first entertaining concepts of such a type surpass rational inquiry.

H-D accounts begin with the hypothesis as given, as cooking recipes begin with the trout. However, recipes sometimes suggest "First catch your trout". The H-D account is a recipe physicists often use after catching hypotheses. However, the conceptual boldness which marks the history of physics shows more in the ways in which scientists *caught* their hypotheses than in the ways in which they elaborated these once caught.

To study only the verification of hypotheses leaves a vital part of the story untold—namely, what were the reasons Kepler, Galileo, and Newton had for thinking their hypotheses would be of one kind rather than another. In a letter to Fabricius, Kepler underlines this.

> Prague, July 4, 1603
>
> Dear Fabricius,
>
> . . . You believe that I start with imagining some pleasant hypothesis and please myself in embellishing it, examining it only later by observations. In this you are very much mistaken. The truth is that after having built up an hypothesis on the ground of observations and given it proper foundations, I feel a peculiar desire to investigate whether I might discover some natural, satisfying combination between the two. . . .

If any H-D theorist has ever sought to give an account of the way in which hypotheses in science *are discovered,* Kepler's words are for him. Doubtless H-D philosophers have tried to give just such an account. Thus, Braithwaite writes: "Every *science* proceeds . . . by thinking of general hypotheses . . . from which particular consequences are deduced which can be tested by observation . . .", and again, "Galileo's deductive system was . . . presented as deducible from . . . Newton's laws of motion and . . . his law of universal gravitation . . ." (*Op. cit.,* pp. xv, xi, 18.)

How would an H-D theorist analyze the law of gravitation?

1. First, the hypothesis H: that between any two particles in the universe exists an attracting force varying inversely as the square of the distance between them ($F = \gamma Mm/r^2$).

2. Deduce from this (in accordance with the *Principia*)
   a. *Kepler's* Laws, and
   b. *Galileo's* Laws.

3. But particular instances of a. and b. square with what is observed.

4. Therefore H is, in so far, confirmed.

The H-D account says nothing about how H was first puzzled out. But consider why here the H-D account is *prima facie* plausible.

Historians remark that Newton's reflections on this problem began in 1680 when Halley asked: If between a planet and the sun there exists an attraction varying inversely as the square of their distance, what then would be the path of the planet? Halley was astonished by the immediate answer: "An ellipse". The astonishment arose not because Newton *knew* the path of a planet, but because he had apparently deduced this from the hypothesis of universal gravitation. Halley begged for the proof; but it was lost in the chaos of Newton's room. Sir Isaac's promise to work it out anew terminated in the writing of the *Principia* itself. Thus the story unfolds as an H-D plot: (1) from the suggestion of an hypothesis (whose genesis is a matter of logical indifference, i.e., psychology, sociology or history) to (2) the deduction of observation statements (the laws of Kepler and Galileo)—which turn out true, thus (3) establishing the hypothesis.

Indeed, the entire *Principia* unfolds as the plot requires—from propositions of high generality through those of restricted generality, terminating in observation-statements. Thus Braithwaite observes: "Newton's *Principia* [was] modelled on the Euclidian analogy and professed to prove [its] later propositions—those which were confirmed by confrontation with experience—by deducing them from original first principles . . .". (*Scientific Explanation,* p. 352.)

Despite this, the orthodox account is suspicious. The answer Newton gave Halley is not unique. He could have said "a circle" or "a parabola", and have been equally correct. The general answer is: "A conic section". The greatest mathematician of his time is not likely to have dealt with so mathematical a question concerning whether a formal demonstration is possible with an answer which is but a single value of the correct answer.

Yet the reverse inference, the *retro*duction, *is* unique. Given that the planetary orbits are ellipses, and allowing Huygen's law of centripetal force and Kepler's rule (that the square of a planet's period of revolution is proportional to the cube of its distance from the sun)—from this the *type* of the law of gravitation can be inferred. Thus the question "If the planetary orbits are ellipses what form will the force law take?" invites the unique answer "an inverse square type of law".

Given the datum that Mars moves in an ellipse, one can (by way of Huygen's Law and Kepler's Third Law) explain this uniquely by suggesting how it might follow from a law of the inverse square type, such as the law of universal gravitation was later discovered to be.

The rough idea behind all this is: Given an ellipsoidal egg-shell, imagine a pearl moving inside it along the maximum elliptical orbit. What *kind* of force must the egg-shell exert on the pearl to keep the latter in this path? Huygen's weights, when whirled on strings, required a force in the string, and in Huygen's arm, of $F_{(k)} \propto r/t^2$ (where r signifies distance, T time, and k is a constant of proportionality). This restraining force kept the weights from flying away like stones from David's sling. And something like this force would be expected in the egg-shell. Kepler's Third Law gives $T^2 \propto r^3$. Hence $F_{(k)} \propto r/r^3 \propto 1/r^2$. The force the shell exerts on the pearl will be of a kind which varies inversely as the square of the distance of the pearl from that focus of the ellipsoidal egg-shell where the force may be supposed to be centred. This is not yet the law of gravitation. But it certainly is an argument which suggests that the Law is likely to be of an inverse square type. This follows by what Peirce called 'retroductive reasoning'. But what *is* this retroductive reasoning whose superiority over the H-D account has been hinted at?

Schematically, it can be set out thus:

1. Some surprising, astonishing phenomena p_1, p_2, p_3 . . . are encountered.[3]

2. But p_1, p_2, p_3 . . . would not be surprising were an hypothesis of H's type to obtain. They would follow as a matter of course from something like H and would be explained by it.

3. Therefore there is good reason for elaborating an hypothesis of type H—for proposing it as a possible hypothesis from whose assumption p_1, p_2, p_3 . . . might be explained.

This is a free development of remarks in Aristotle (*Prior Analytics,* II, 25) and Peirce. (*Collected Papers,* Vol. I, 188. Peirce amplifies: "It must be remembered that retroduction, although it is very little hampered by logical rules, nevertheless is logical inference, asserting its conclusion only problematically, or conjecturally, it is true, but nevertheless having a perfectly definite logical form.")

How, then, would the discovery of universal gravitation fit this account?

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[3] The astonishment may consist in the fact that *p* is at variance with accepted *theories*—as, e.g., the discovery of discontinuous emission of radiation by hot black bodies, or the photoelectric effect, the Compton effect, and the continuous β-ray spectrum; or the orbital aberrations of Mercury, the refrangibility of white light, and the high velocities of Mars at 90°. What is important here is *that* the phenomena are encountered as anomalous, not *why* they are so regarded.

1. The astonishing discovery that all planetary orbits are elliptical was made by Kepler.

2. But such an orbit would not be surprising if, in addition to other familiar laws, a law of 'gravitation', of the inverse square type, obtained. Kepler's First Law would follow as a matter of course; indeed that kind of hypothesis might even explain why (since the sun is in but one of the foci) the orbits are ellipses on which the planets travel with non-uniform velocity.

3. Therefore there is good reason for elaborating hypotheses of this kind further.

This says something about the rational context within which an hypothesis of H's type might come to be "caught" in the first place. It begins where all physics begins—with problematic phenomena requiring explanation. It suggests what might be done to particular hypotheses once proposed, viz., the H-D elaboration. And it points up how much philosophers have yet to learn about the kinds of reasons scientists might have for thinking that one kind of hypothesis may explain initial perplexities—why, e.g., an inverse square type of hypothesis may be preferred over others, *if* it throws initially perplexing data into patterns within which determinate modes of connection can be perceived. At least it appears that the ways in which scientists sometimes reason their way *towards* hypotheses, by eliminating those which are certifiably of the wrong type, may be as legitimate an area for conceptual inquiry as are the ways in which they reason their way *from* hypotheses.

Recently, in the Lord Portsmouth collection in the Cambridge University Library, a document was discovered which bears on our discussion. There, in "Additional manuscripts 3968, No. 41, bundle 2", is the following draft in Newton's own hand:

> ". . . And in the same year [1665, twenty years before the *Principia*] I began to think of gravity extending to ye orb of the Moon, and (having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere), from Kepler's rule . . . I deduced that the forces which keep the planets in their Orbs must be reciprocally as the squares of their distances from the centres about which they revolve . . ."

This MS corroborates our argument. ("Deduce", in this passage, is used as when Newton speaks of deducing laws from phenomena—which is just what Aristotle and Peirce would call "retroduce".) Newton *knew* how to estimate the force of a small globe on the inner surface of a sphere. (To compare this with

Halley's question and our pearl-within-eggshell reconstruction, note that a sphere can be regarded as a degenerate ellipsoid, i.e., where the foci superimpose.) From this, and from Kepler's rule $T^2 \propto r^3$, Newton determined that, whatever the final form of the law of gravitation, it would very probably be of the inverse-square type. These were the reasons which led Newton to think further about the details of universal gravitation. The reasons for accepting one such hypothesis of this type as a law are powerfully set out later in the *Principia* itself—and they are much more comprehensive than anything which occurred to him at this early age. But without such preliminary reasoning Newton might have had no more grounds than Hooke or Wren for thinking the gravitation law to be of an inverse square type.

The morals of all this for our understanding of contemporary science are clear. With such a rich profusion of data and technique as we have, the arguments necessary for *eliminating* hypotheses of the wrong type become a central research inquiry. Such arguments are not always of the H-D type; but if, for that reason alone, we refuse to scrutinize the conceptual content of the arguments which precede the actual proposal of definite hypotheses, we will have a poorer understanding of scientific thought in our time. For our own sakes, we must attend as much to how scientific hypotheses are caught, as to how they are cooked.

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IMPERATIVES AND INDICATIVES (I)

By P. C. Gibbons

It is the principal contention of this paper that the indicative mode, the informative or fact-stating way of using language, is not just one among a number of co-ordinate and relatively autonomous ways of doing so, but is the basic, fundamental way of using it upon which all its other uses depend. In the first part I shall make certain quite general points about meaning, but the main body of the paper will be devoted to the consideration of what might be thought a particularly recalcitrant counter-example. In the second part of the paper I shall be concerned to argue that there is no separate logic of imperatives, that from the point of view of logic imperatives *are* indicatives. In the third part I shall turn to a consideration of the peculiarities of imperatives, and shall argue that the questions which arise in this discussion are not primarily logical at all but sociological.

The Ubiquitousness of the Indicative

Attention may first be drawn to the distinction between what I *say* and what I *mean,* or, what comes to the same thing, between what *I* mean and what *what I say* means. I *may* say what I mean—that is, succeed in saying what I mean—but equally I may fail to do so. When I say "What I meant was so-and-so" I am certainly not saying that what I *said* meant so-and-so; on the contrary, I am admitting that what I meant I did not say, that what I said was not *what* I meant and hence not what I meant to say. There is a use of language, that is to say, in which I simply say something, and in which the meaning of what I have said is to be determined irrespective both of what I meant and of what I meant to say. It is this fact which makes appropriate such remarks as "Well, that's what you *said*", where I insist that you have said something and stick to it that it is this rather than that that you did say. It is only if you allow the existence of this *dictum,* which is what is said or stat*ed*, that you can go on to make sense of the distinction between your having said (your happening to have said) what you meant, and your not having done so. This dictum, this something said or stated, is not, of course, identical with the sentence used, but is a proposition—is what the sentence in use states, and what, by uttering it, you yourself have, willy-nilly, stated. It is important here not to confuse what the sentence in use does, just as a sheer matter of linguistic (and hence social) fact, state, and what it is used *to* state. The question raised is precisely not one about individual purpose but about what, within a given group of language-users and within a given society or part of society, the given sentence in the given context just does mean. This is at least part of what is involved in maintaining that languages are social, *viz.* that sentences in them have meaning independently of the purposes of particular users. A language as a system of meanings must exist prior to the individual uses that are made of it if these uses themselves are to be possible. We do not create meanings, we discover them, and if anything sensible can be said about the origins of language, the same will hold there too. In sum, then, we may say that a sentence in use will, unless it is nonsensical, have as its *primary* meaning some proposition, where what I mean by a proposition is just this primary meaning, and what I mean by calling it primary is just that it must have such a (prior, social) meaning if it is to have any meaning at all.

A sentence, besides meaning some proposition, may mean all sorts of other things as well. It may mean something *to me* (or to us) that it does not mean to you, or to you that it does not to me; and its utterance may be meant *to do* something, to have some effect. These are or are among its *secondary* meanings. Its primary meaning depends of course upon its social context: what means something in one language (and hence for some one people or social group) will generally mean nothing at all in some other language (and hence for other social groups). However, secondary meanings are socially determined in yet a further sense: they depend upon the causal and other *relations* of the sentence in question to the feelings and attachments of the utterer and to the activities of particular groups within the given group of language-users. The relations in question may be either those of the sentence or of its words to such feelings and activities, or else less directly those of other propositions to such feelings and activities —those, for instance, of the implicates of the original proposition. It is these relations that give utterances their point, and that enable us to use sentences to some purpose, and from the point of view of a social understanding of language it is these that are important. However, and this is more important still: it is not these that determine whether a sentence is meaningful or not; a sentence is meaningful only if and in so far as it has primary meaning, and otherwise not. Point and meaning are quite distinct. A remark may be quite pointless, and yet be a genuine remark—we may still be saying something, though what we say be not worth saying. We should perhaps distinguish still further between point and worthwhileness, since no remark, not even a nonsensical one, is altogether devoid of point, at least from a psychoanalytical point of view, and whether a remark is worth making or not itself depends upon one's point of view.

The relevance of all this to my main contention may be stated quite simply. Primary meaning, I should maintain, is always indicative; it is only secondary meaning that is not. The point of an imperative no doubt resides, at least commonly, precisely in its imperativeness; its meaning on the other hand does not. It may well be of the first importance, both from the point of view of theory and from that of practice, to see the point of a remark—or, for that matter, and perhaps more obviously, of a question or of a command. But if the question is of theory, then it is every bit as important to ask for which theory it is that the point of that remark or command or question is of concern. The answer, it seems to me, will always be: for sociology or for psychology; never for philosophy, let alone for logic.

If the confusion of these other fields with philosophy were all that was involved, it might not matter so much; it might only mean that a large number of people were doing good (or bad) work in these fields under the mistaken impression that it was philosophy that they were pursuing—as is, I believe, the case. As we should expect, however, the confusion in fact goes deeper, and points and purposes are only too often treated as if they were constitutive of meanings. This is at least sometimes the case with those who employ the notion of the "use" of a sentence. I do not wish to deny—would indeed wish to emphasise—that utterances have their meanings as social acts in a context of other activities—and this is one thing that I take also to be emphasised by those who employ this notion. When, however, the notion of use is identified with the notion of point or purpose—with what a sentence is used *for, to do,* or *to get done*—then I see in this only an ill-judged revival of pragmatism. In one sense, to say of a sentence that it is being used (or is in use) on a given occasion is just to say that an utterance of it occurs together, perhaps, with whatever not narrowly linguistic concomitants are necessary, for instance, to establish its reference. It is a quite different thing to say of it that its utterance serves some purpose, since it may fulfil all the above requirements and yet still not do this. Moreover, it may serve some purpose despite the fact that, strictly, it does not fulfil all those requirements: its utterance may serve quite satisfactorily to give a warning, for instance, despite the fact that its reference is not definitely established at all. However that may be, the concentration of attention upon secondary meanings goes all too often either with an identification with them of what I have called primary meanings or (what comes to the same thing) with the denial that the latter occur at all.

So far as secondary meanings are concerned, it is certainly tempting to revert to the old-fashioned analysis of them in terms of intention, to account at least for them in terms of what I intend in saying what I do. For myself, I am inclined instead to reverse this procedure, and to analyse intention in terms of meaning rather than vice versa. The significance of an utterance, the meaning it has in a given context, is a matter of the part it has to play there, of its conditions and consequences, of its causes and effects. It is in terms of these that it can be a sign of other things, and hence in terms of these that it is capable of signifying, of being significant, of having meanings. Inevitably, of course, it will have many such connections; inevitably, then, it will be a sign not of one but of many things, inevitably have not one but many possible meanings. Like all symbols it is at least potentially multivalent and generally actually so as well. It is this that makes possible the distinction between what it means to me and what it means to you, between what it means to us and to them. What I mean by something, then, is what that something means to me; it is a selection from all the possible secondary meanings that something has, a selection to which additions may accrue as I discover more of its connections and from which subtractions may be made as my circumstances alter or my interests change. It is in terms of this meaning of 'meaning', I believe, that an account of intention is to be given. What I intend by a remark is that part of its meaning to me which affects the future and of which I am aware at the time of utterance. Its meaning to me may subsequently increase, but this is not so with intentions; imputed intentions I can subsequently disavow on the ground of ignorance or unawareness, and cannot, as it were, posthumously discover, as I can discover new meanings. I can discover, that is to say, what a given remark meant to me even after I have made it—can discover, that is, its place in my mental life, its connections with other parts of that life and with the activities going on around me. One often surprises oneself by what one has said. I can also, of course, become aware later what my intentions were. But whereas to discover such meanings is only to discover these connections, to discover my intentions is to discover a (prior) awareness of these connections. Of such awarenesses themselves I may have been or remain unaware; in this case my intentions were unconscious—were awarenesses of which I was not in turn aware. In this sense, then, I may later discover (or fail to discover) what were in fact my intentions; these latter, however, are in no way altered by such a discovery, but were what they were whether I was or became aware of them or not. In this they contrast with meanings, which do so alter as I discover more.

The account of intention that I have tried to adumbrate here is not one which I should wish to confine merely to linguistic intentions. There is this difference, perhaps, that my linguistic intentions I do not ordinarily announce in advance of my utterances, but elucidate, if at all, only afterwards. My non-linguistic intentions, on the other hand, I do quite commonly announce, if only to myself. The meaning of intention is, however, in each case the same. If I say that I intend to do something, then in my view what I am saying is simply that, as I read the signs, I will do something—that my present state of mind signifies or means to me my doing that thing in the future. About this I may, of course, be mistaken, not only because I may be prevented from doing it but because I may misread the signs. It may be objected that into intentions there enters an element of determination, of will, of which my account takes no notice. On the contrary, why should we not read "I am determined to do so and so" in the sense which "determined" ordinarily has? Why should my present state of mind not signify to me my doing something in terms of its causal connections? All that is missing from this account is the sense of strain involved. Here it is worth noting that we do not ordinarily predict our routine activities, that what in fact provokes a statement of intention is not any certainty but a doubt about the future, and that this doubt is commonly one rooted in disinclination. What I say (emphatically) that I *will* do is what, normally, I cannot be at all sure of doing. When I say that I *will* do something it is in fact because it will not be willingly that I do what I intend to do—not, that is, without effort, strain, mental conflict and internal resistance. That is, if I do it; one is only too often wrong about one's intentions—about what is intended or meant by a given state of one's mind.

The relevance of all this is to the study of secondary meanings—of points and purposes, and of the roles that utterances play in our social and psychological life. Proper as such studies are, I hope it is clear why I do not regard them as logical, let alone as offering any real foothold either to teleology or pragmatism. From the point of view of this paper, however, the important point is the distinction that it enforces between primary and secondary meaning. Later in this paper I shall have occasion to contend that even socially the part played by indicatives is more fundamental than that played by imperatives. Here my contention is that indicating or informing is not, in the same sense that commanding or ordering is, a role or function of sentences. The causes, effects and conditions of my saying something are all of them to be distinguished both from my saying it and from what I say. It is in terms of a selection from among the former that we determine the role of a saying; but to refer to the indicative role of a sentence is to refer to the saying itself—to the *dictum,* to what is *said*—and not to either its conditions or its consequences. It is not just that one cannot command without indicating, but that indicating is not like commanding, insulting, warning, etc., a role that sentences play, something that they *do,* a relation into which they enter; indicative is what they *are* and must be if they are to have any of these roles, do any of these other things at all. For those who collect category mistakes I offer the contrary view as a possible example.

The Indicativeness of Imperatives

So far I have simply taken it for granted, without argument, that the primary meanings of sentences are indicative. No doubt something could be done to establish this *a priori*; for instance, by reference to the logical priority of the verb "to be" over all other verbs, and by arguments not altogether dissimilar from those employed by Kant when he discusses objectivity in his Transcendental Deduction of the Categories.[1] Such knock-down arguments may seem far from irresistible, however, in the face of apparently immovable objects. What I propose instead, therefore, is to attempt to show that in at least one and that, I should have thought, a conspicuously obstinate case, that of imperatives, this immovability is in fact only apparent. I shall, then, in this part of the paper, attempt to show that the primary meanings of imperatives are indicative.

R. M. Hare in his book *The Language of Morals* considers and dismisses two proposed forms of reduction of imperatives to indicatives. The first proposal is that we should expand imperatives by introducing the words "I want you to" before every command, thus replacing "Do this" with "I want you to do this". This obviously will not do: it is only exceptionally that your wish is my command, and in the face of a mere statement of your desires it is most unlikely and would be most inappropriate that I should ask: "Is that an order?" Clearly, it is not. It is one thing to tell someone what I require, another to require something of him, and yet a third to require him to give it to or procure it for you. Moreover the statement of my wishes is not only superfluous but irrelevant: they need not be my wishes at all. My wish is not even *my* command. I may well be obliged to order what I do not desire in the least; even to issue commands that I would prefer to have disobeyed. It would be very odd indeed if genuine commands regularly took the form of lying falsehoods. It would, of course, have been more plausible to have proposed an impersonal rendering of the form "It is required that you do this". This, however, is also misleading. First, it presupposes that we could in principle specify who or what requires your doing this, and for what. But it is one of the distinctive features of orders as such that they may be completely arbitrary—in giving an order I am not only unlikely to encourage such questions, I make no pretence to be able to answer them. Again, it is I who give the order, and hence, in one sense of 'require', I who require your doing this. But it is one thing to order you to do it, another to say that I order you to do it. It is true that "Do this" and "I order you to do this" may both function in the same way, and that the longer form merely repeats and reinforces the shorter; but in any case such a translation completely fails as an analysis, since what we really want to know is what it is to order, and merely to insert the words "I order you to" into the order itself goes no way towards explaining this.

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[1] *Critique of Pure Reason* B 141-2.

The second form of reduction that Hare considers is connected with this last, *viz.* the "Do this—or else" reduction: the reduction that treats all commands as disguised and incomplete hypotheticals or disjunctives. Superficially this is a most attractive reduction, at any rate to those who have no use for a categorical imperative. Its appeal, however, is essentially rationalistic: it suggests that an element of calculation is involved in every act of obedience, a weighing of consequential rewards and punishments. This, however, is simply not true; it is not necessarily true even that he who gives the order *could* set out an array of inducements positive and negative. In fact he would rarely dream of doing so: it would be bad for discipline. A dark threat is ever the best. The suggestion behind the analysis is that it is only adventitiously that commands as hypotheticals or disjunctives are left incomplete: that the "or else" in "Do this, or else . . ." precedes a genuine gap, open to genuine completion. It may do: not all our threats are empty threats, not all our promises worthless. But to give an order is not necessarily to threaten or promise at all; it is not even true that every order is associated with threats and promises. It may be that ordering people about generally does go along with the attempt to bully and bribe them, but if so the connection is synthetic. The threat or promise is something extrinsic to the order itself: it supplies us with a reason for obeying, and that there is any reason for obeying it is certainly not entailed by the fact that an order is given. "Do this—or else" is often just bluff; so is the analysis that universalises this form.

However, misleading as is this analysis as it stands, it is right in so far as it reminds us that commands may be associated with reasons. All that is required is that we should excise the empty formula "or else" and the often dubious reason that we would have to supply to fill it. "You will do this or else" and "if you do this, then" now become simply "you will do this", which is the essentially imperative remainder from the hypothetical command. This has the two advantages that it is a form which we do actually use for giving orders, and that it can be rejoined to threats and promises by means of 'either-ors' and 'if-thens', bearing in mind that the objection to a reference to threats and promises is not made on the ground that threats and promises are irrelevant or inappropriate, but only that they are superfluous or inessential—not part of the command itself. There is not the faintest objection to my filling in the context of "You, John, will lay the table" by saying "Either you, John, will lay the table, or I'll give you a thick ear" or "If you, John, will lay the table, I'll let you off the washing up", but only to your imagining that "You, John, will lay the table" *means* either one or other of the two associated sentences. How, after all, could one possibly know which (if either) of them was meant?

It would no doubt be argued at this point that, unobjectionable as it is in itself, to translate "Do this" by "You will do this" hardly effects a reduction, since either the resulting formula remains itself an imperative, in which case no reduction takes place, or else it is not imperative, in which case it can hardly be equivalent to the formula that it is intended to replace, which ineluctably is. A momentary escape from this dilemma immediately presents itself in the distinction between grammatical form and function: one might claim to have effected an admittedly trivial but nonetheless real reduction if one could show that there is no sentence that has the imperative function but can be replaced without remainder by one having the indicative form. It is clear that not only sentences having the imperative form, like "Lay the table", but also, for instance, questions like "Will you lay the table?" may have this function. So, too, can seeming indicatives, like "You will lay the table", and there appears to be nothing to stop us from singling out this form as the most convenient for certain purposes: it would be a natural preference in the logician. Equally, of course, if these three forms—and possibly others—have all really the same content, and *can* have the same function, there is nothing to prevent us from singling out any one of the others. If they are really equivalent, then we cannot lose, in whichever direction we move; but then again, neither can we gain. It is not really the formal question in which we are interested: it is that of function. What it is really required that we show is that a sentence *imperative in function,* be its form what it may, can always be replaced by one *indicative in function,* be its form what it may. It does not suffice that we should be able to replace "Do this" with "You will do this" where both remain imperative; it is necessary that we be able to replace "You will do this", where the sentence is used imperatively, with an indicative use of the very same sentence. It is after all to precisely this difference in use that the logico-linguist or linguistic logician would wish to draw our attention; it is precisely to the ignoring of such differences that he points as evidence of the heavy-handed blundering of traditional logicians.

Now precisely what I wish to maintain is that the "You will do this" with which I wish to replace "Do this" is indicative; but in saying this I do not wish to deny that it is also imperative, or to assert that its imperativeness can in some way be done away with. Strictly, then, the move that I wish to make is in one important sense not a reductive one at all; it is more like an attempt at definition. If there is anything to being imperative, it cannot be done away with; if there is not, there is nothing to be reduced; and I do not wish to maintain that people have been universally mistaken in thinking that there is. What I wish in effect to maintain is that imperatives are a species of indicatives, most conspicuously of second person future indicatives. Hence, far from wishing to deny that they have a distinctive use, I wish squarely to face the question: what is the difference between them and other indicatives? The reduction involved is one of categories, of logics.

What then is to prevent us maintaining that imperatives are indicatives—that, for instance, "You will do this", issued as a command, is nonetheless indicative? It will not do simply to say: because it is imperative, since whether something can be both imperative and indicative at the same time is precisely what is at issue. Put it that ordering and indicating are two different functions, roles or uses that one and the same form of words may have—why should it not have them simultaneously? Fond of talking of logics, linguists are over-inclined to parsimony when it comes to the plurals of such words as 'use' and 'point'; they are too easily satisfied that they have hit upon *the use* or *the point* of a given utterance. Have not single utterances many uses, many functions, many points, many bearings in the complex situations in which they actually occur? Why should not one and the same utterance both command and indicate? After all, both are in some sense relations that the utterance has to its context, and it requires special argument to show that it cannot do both.

It also takes special argument to show that it can. Consider the sentence: "The class will now dismiss". It is clear that such a remark might be addressed by a teacher to his class, and would then function as a command. Equally it might be the Headmaster's aside to a visitor, in which case it would function for him as a prediction. But now why, if addressed by the teacher to his class, should it not function simultaneously for them as a command, and for the visitor as a prediction? Why, if it comes to that, should it not function also for the class as a prediction? Why, indeed, should we not say that, whether or not it functions for anyone as such, it *is* a prediction?

There are as I see it two reasons against this, neither of them adequate. In the first place, it would be socially obtuse to the point of outrageousness. Suppose a schoolboy, told that his class would dismiss, naïvely to inquire of his instructor: "Do you really think so, Sir?" In one sense, no doubt, it would be grossly impertinent of him; but the point is one of etiquette, not of logic. We have only to imagine the class in a state of insubordination and the query would appear as by no means inept. We might still be inclined to say that it revealed a deliberate misunderstanding of the utterance by failing to recognise its purpose: its whole point, we are inclined to say, is that it is given as an order. We show the same obtuseness when we fail to recognise an insult for what it is, or a snub or a rebuff. But is this, in fact, always obtuseness? It may be the mark of sophistication.[2] And is it really to misunderstand, any more than it is to misunderstand someone if we seize upon those implications of his remarks that he has overlooked and would perhaps prefer to go on overlooking? Certainly it is not kind, and it may be discourteous; but that, for the moment, is hardly our concern. In our tougher moments we rightly insist on what a person says, and when he claims not to have meant what he said, upon his saying what he does mean. If he is to plead that distinction in his defence, then in fairness we are bound to go by what he does say. And this seems to me to apply here: the impish schoolboy and the toughminded logician are at one on this. The master *said* the class would dismiss, but he was wrong: it will not.

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[2] The point is neatly exampled in the following exchange from Miss Compton-Burnett's *A House and Its Head*:

> " 'The scene makes me hysterical, Father. I shall gather up my presents, and bear them to safety.'
>
> " 'You will sit down, Nance, and remain in your seat' said Duncan, accurately forecasting what would take place."

The author's comment is, of course, ironic. However, understatement has this advantage over overstatement that whereas the latter sins against the injunction that we should tell nothing but the truth, the former does not, but only against the injunction that we should tell the whole truth; and the irony here takes the form, not of opposing forecasting to something which is not forecasting but of opposing mere forecasting to what is by no means mere forecasting.

What is being maintained in this paper, of course, is not that to order is *merely* to predict, but that it *is* to *predict*; alternatively, that an order is merely a prediction which . . . (or such a prediction as . . .) where what is merely a prediction *which* . . . is certainly not *merely a prediction*.

Mere callousness, however, will not help us with the second set of objections, which is as follows: it is inappropriate, even absurd, to treat as genuine a prediction that it is within our power to verify or falsify as we see fit. If it lies within my power either to obey or disobey an order—and it must be if it is an order worth giving, since it is no use ordering someone to do what he can't do, and there is no point in ordering him to do what he must do—then whether I do what I am ordered to do *must* be unpredictable. Moreover, what I do I may do as a consequence of being ordered to do it, and the action I thus perform will commonly be one that I would not otherwise perform. This is in fact one of the distinctive features of orders, but it gives rise to the curious position in which the making of the supposed prediction is one of the conditions of its being fulfilled. Equally curious consequences arise in the case of contrariness, where the making of the supposed prediction (the giving of the order) would be a sufficient condition of the order's not being obeyed, and thus of the prediction's *not* being fulfilled. Finally, "You will do this" becomes, for the one who is commanded, "I will do this", and it is sometimes alleged that it is at least immoral to make first person predictions in so far as it is to abdicate one's responsibilities. Granted the possibility that a command might serve for a third person as a prediction it will be contended that for the second person, the person commanded, it could not.

Considerations such as these are not, of course, novel; in fact, in part they constitute merely a restatement of the by now familiar paradoxes of prediction. On the face of it, however, self-verifying and self-stultifying predictions do occur, as is alleged to have been the case in a certain presidential election in America, so that unless we are prepared to say that these also are *ipso facto* not genuine predictions, there would appear to be nothing in this particular objection. No doubt it makes life difficult for some forecasters, and there exist correspondingly soft options for others, but there seems to me no logical difficulty about the situation in which one cannot *publish* a forecast without its going wrong, and equally no logical difficulty about the situation in which one cannot get something done by ordering it—that, surely, should be a commonplace, however irksome a one for dictators and moralists.

As for the rest of these arguments, they reduce once more, it seems to me, merely to the view that determinism is false and immoral, and are to be met in precisely the same way as such charges are always to be met. The sense in which it must lie within my power to obey or not obey, and in which I cannot evade responsibility for my actions in doing one or the other, need not for a minute preclude a causal account of what I do do. It is sufficient evidence that I can do something that I sometimes do do it, and that I cannot do it, that I sometimes do not, and of course there is nothing in this to show that I will or will not do it. On the other hand this hardly goes to show that there are no sufficient conditions of my doing or of my not doing what the order bids me do, but the order may itself, in the given context, be sufficient to ensure that I do or fail to do what I am bidden. That I can do or not do something does not prove that I can do or not do it now; indeed that I don't do it now if I don't is the very best evidence that I can't, and that I do do it now, if I do, is the very best evidence that I must, in the simplest causal sense. As for responsibility, that rests upon me so long as anyone cares to take note of the fact that it is I who obey or disobey, and to assign responsibility to me.

My conclusion, then, is that there is no more to these arguments than there is to the argument that psychoanalysis is impossible, or that, if it is not, then self-analysis is. No doubt many a prediction is false, wrong-headed and self-stultifying; no doubt the same is true of orders. So, if I may ask, what?

For some, clearly, the most shocking and retrogressive implication of my thesis will be the one that commands can be true or false; for others, the consequence that the mandatory system is a magical one. Some philosophers recommend nothing if not our emancipation from a narrow concern with truth and falsity, and for them orders are near enough paradigm cases of sentences to which such notions do not apply. To this contention I shall be returning shortly *apropos* of contradiction; meanwhile let me say that it seems to me to show just the same sort of failure to appreciate the way things stand to give an order that is bound to be disobeyed as to prophesy a win for a lame horse; it shows a precisely parallel wrongheadedness and wishfulness of thinking. I am even prepared to say that an order can be a lie. Order someone to do something that will not be done, whether because he cannot do it or because you know he is going to disobey you, and you may arouse false hopes and expectations as surely as with a misleading promise: in the one case in the person to whom you give the order, in the other in any bystander who happens to overhear you. If you do this deliberately, then what you do amounts to lying. The "You will do so-and-so" form only serves to make explicit what is in any case involved here.

As for commands as incantations, it is surely clear that in the mandatory approach we have not merely meagre remnants but a towering edifice of magical exhortation. On my view, to give a command is to try to make something happen by saying that it will; but what is it on anybody else's? We regard it as rank superstition to order mountains to move or rain to fall—or at least some of us do—but we find nothing absurd in the indiscriminate issuing of precepts—or at least some people don't. But surely it is in fact as absurd to give orders to a person who you know does not, either because he cannot or because he will not, obey them as to summon lightning or foretell a win for a lame horse? Of course, it is not absurd to give orders to the right people, but then neither is magic always absurd; cast your spells in the right places and it is surprising how effective they can be. Point a bone in certain parts of Australia and you can count on your victim's death; dispense a love potion in certain parts of Africa and you can more or less count on your own. As mandators we can rely for the most part only on rather less dramatic effects; but our reliance is no more if no less well based.

(*To be concluded*)

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OBSERVATIONS ON THE DISTRIBUTION AND "SIGNIFICANCE" OF TERMS IN PROPOSITIONS

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I

The word "significance" is used here to indicate something parallel to distribution, something related to the intension of a term as the latter is to its extension. I wish to present a certain type of interpretation for such familiar statements as "The subject of an A proposition is distributed, its predicate is undistributed" and also to set out parallel statements, e.g. "The predicate of an A is significant, its subject is not significant", with corresponding interpretations. The treatment of distribution statements comes from Professor Anderson, who regards them as statements of the validity or invalidity of certain syllogisms,[1] though they appear to be concerned with propositions taken by themselves. The theory stands out most clearly in opposition to theories of a nominalist kind which reduce universals to certain of their actual implications, but it is also distinct from any idealist notion of propositions having to be understood as elements in an argument, having only quasi-independence. In part III I suggest that such reduction of the traditional four forms of proposition is a natural result of the usual treatment of distribution, and some examination is made of the partial reduction which Mill presents in his discussion of the functions and logical value of the syllogism. More recent and more systematic reductions are not examined, though it will be clear that the treatment of intension as on a level with extension is incompatible with a number of these.

It is the equality of intension with extension that I am chiefly concerned with in proposing a notion of "significance" as parallel to that of distribution. If we can say that when a term is distributed in a proposition it is "not significant" there, and that when undistributed it is "significant", then any rule for validity couched in terms of distribution could be replaced by one referring to "significance". This means that the distribution rules do not show any primacy for extensive considerations, even if we think it convenient to use these rules rather than ones involving intensive considerations. But recognition of the equality of extensive and intensive considerations is of little use if both extension and intension are treated as relating to words and if no objective term is recognised as possessing extension and intension. In part IV I deal briefly with Keynes's theory of a four-fold "reading of propositions" which treats extension and intension in that way. In Anderson's theory of distribution propositions and terms are taken objectively: so of course is the extension of a term and in speaking of the "significance" of terms I shall also be taking their intensions objectively. It is unfortunate if the word "significant" suggests something concerned with words but I am using it as a technical expression.

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[1] This theory is part of a general view, a development of traditional logic, which Anderson has expounded for many years at the University of Sydney in courses on elementary logic. Only parts of this view have as yet been published and my references are to lectures of the early thirties.

I wish to say that, just as subjects of universals and predicates of negatives are distributed while subjects of particulars and predicates of affirmatives are not, so subjects of particulars and predicates of affirmatives are "significant" while subjects of universals and predicates of negatives are not. Such statements about "significance" will require an interpretation by reference to syllogisms[2] just as those about distribution do, according to Anderson, and these interpretations are needed if one is to see clearly the difference between saying that a term is undistributed in a proposition and saying that it is "significant" there or between saying that it is distributed and saying that it is "not significant". However, it is possible to get a more rough and ready interpretation for both kinds of statement. Thus on the side of distribution we might say that an A proposition "applies to its subject in its whole extension", and on the side of "significance" that it "carries with it or signifies its predicate in its whole intension". We might even say that the A "*applies* to its predicate in its whole intension", but the word "applies" suggests extensive substitution and some other word is preferable, even if "signifies" is not the most suitable. We seem to lack any word or simple phrase parallel to "applying to", the point no doubt being that attention has been fixed on those implications where it is possible to say that one of the premises applies to a term appearing in another premise. On the other hand, "significant" is in some ways a useful parallel to "distributed" since significance is commonly linked with intension or connotation. There may be something odd about saying that a term is "not significant" in a certain proposition, if we do not take "significant" as a technical expression, but equally if we take "distributed" in an everyday non-technical way there is something queer about saying that a term is not distributed in a given proposition.[3]

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[2] The statements were in fact arrived at from the consideration that there is a set of four valid syllogisms corresponding to the set relevant to distribution. Material of Anderson's on the possibility of regarding syllogism as a process of substitution suggested the correspondence. He had pointed out that AAA in the first figure (which is common to the two sets) may be regarded either as an extensive substitution or as an intensive one and that OAO in figure III, the one valid mood with a negative premise not distributing the middle term, is a process of substitution, though this is of an intensive, not extensive, character.

The following are suggested as rough explanations of the two technical expressions: (a) a term's being distributed in a proposition is a matter of that proposition applying to it in its whole extension; (b) a term's being significant in a proposition is a matter of that proposition signifying it in its whole intension. These explanations are filled out in part V, but, as I there deal separately with distribution statements and statements concerning significance, I shall indicate here the symmetrical character of the results, taking the A proposition as an example. The syllogisms, valid and invalid, that are relevant to distribution statements are those directly describable as extensive substitutions, and the ones relevant to statements regarding significance are those directly describable as intensive substitutions. AAA in the first figure may be described in either way. From it we see that a substitution can always be made without loss of truth in respect of the subject of an A (the subject of the major premise) and equally that one may be made in respect of the predicate of an A (the predicate of the minor premise), but what is substituted in the first case is an extensive value (the subject of the minor premise) while in the second case an intensive value of the middle term is substituted (the predicate of the major premise). If the intensive type of substitution is made in respect of the subject of an A it will sometimes result in loss of truth, and the relevant invalid syllogism is AAA in figure III. Each premise of this gives an intensive value for the subject of the other, but in the conclusion the value substituted is the one obtained from the minor premise and the substitution is made in respect of the middle term as the subject of the major premise. Equally if the extensive type of substitution is attempted in respect of the predicate of an A, it is the invalid syllogism AAA in figure II that is involved. Here each premise gives an extensive value for the predicate of the other but the value substituted in the conclusion is obtained from the major premise, and this is substituted for the middle term as the predicate of the minor premise.

The same situation obtains with each of the other three types of proposition, i.e. when a substitution of the one type may be made in respect of a term without any loss of truth, the other type would involve loss of truth at some point, and equally when the one process is invalid the other is valid. This situation is perhaps more obvious with regard to the other propositions than it is with regard to A, since in their case there are distinct valid syllogisms involved as well as distinct invalid ones, and in each syllogism the premise giving an extensive or intensive value for the middle term stands out as the one A premise. The fact that *some* type of substitution is permissible in respect of a term, however it is functioning,[4] may be of interest quite apart from its relevance to the notion of distribution or any corresponding notion of significance. It is important, though, that the possibility of valid extensive substitution in respect of a term in a proposition is what has been vaguely indicated by saying that the term is distributed in that proposition. And the interesting thing, in my opinion, about the possibility of valid intensive substitution in respect of a term in a proposition is that this could be used wherever distribution is used, e.g. instead of the fallacy of undistributed middle we could refer to one of double significance of the middle term.

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[3] Consider here the "distributive" use of *all* and *some*. If we contrast the distributive and collective use of "all" in statements about armies, we seem to be involved in some contrast between the being distributed and the being collected of the sort of things armies, and *this* distribution would hold even if our term were functioning as predicate of an affirmative or as subject of a particular.

II

In this section I hope to enforce the point that the notion of a term's being distributed or undistributed in a proposition is a technical one requiring more adequate explanation than it usually receives. In the traditional formulae there is no verbal indication of the distinction, as there is for that between affirmative and negative quality, or for that between universal and particular quantity, though a student may get a working grasp of it by reference to the second of these simpler distinctions. Assuming that it is obvious that subjects of universals are distributed and those of particulars undistributed, the status of the predicates of the E and the I could be grasped by calling in the converses of these, where the original predicates appear as subjects respectively of a universal and a particular proposition. There remain the distribution of the predicate of an O and the lack of distribution of the predicate of an A, which both raise a difficulty for dealing with distribution and the lack of it in terms of the quantity of propositions. In the case of the O there is no help from immediate inference and while, as a teaching device, the formula "Some X are not *any* Y" might be used, this does not actually indicate any kind of universal proposition. It is a dangerous device suggesting that there is some Hamiltonian convertible proposition of mixed quantity, and if the A were similarly represented as

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[4] Note the parallel with the possibility of adding either a determinant (to a distributed term) or an alternant (to a significant term).

"All X are some Y" we should be well on the way to Hamilton's schedule of eight propositions. In the case of the A, since there is a converse though not an equivalent one, we might deal with the predicate by reference to this particular converse, where it appears as subject. There is, however, some arbitrariness in this, since there is another non-equivalent implication of the A where its predicate appears as the distributed predicate of an O. This is the partial inverse of the A, which is commonly taken to raise a problem.

Before considering this further, it may be argued that as distribution can be partly grasped by reference just to the quantity of a proposition, so significance can be partly grasped by reference just to quality. Assuming that the predicates of affirmatives are obviously significant and those of negatives obviously not, the conversion of the I and the E would let us see that their subjects are respectively significant and not significant. Again the partial contraposition of the A and the O would show their subjects to be respectively not significant and significant. In this case the partial inverse of the A will not raise any problem, since it is part of the assumption that the predicate of an A is obviously significant and that the predicate of an O is obviously not significant. However the *regular* problem about this partial inversion could be set out in terms of significance as well as in terms of distribution. An illicit process such as the inference appears to involve could be regarded either as the gaining of a distribution or as the losing of significance, the passing from a significant term to that same term not significant. And, as far as the partial inversion of an A is concerned, there may be some advantages in considering an illicit process in this second intensive way.

Commonly, when a problem is found in the partial inversion of an A, it is being taken for granted that the predicate of an A is undistributed and that of an O distributed. The problem is usually set out by reference to a rule that if a term is distributed in a conclusion it should have been distributed in the premise or set of premises. But retention of this rule obscures the real problem. On the face of it the partial inversion of an A shows up a rule that gaining a distribution is always invalid, though it would leave unaffected a rule that this is always invalid in this or that type of inference, e.g. in syllogism or in conversion. We may find a problem in such variation within valid inferences and seek for some difference between the two types of case, but if we treat gaining a distribution as never valid we have to proceed in some such way as Keynes does in order to retain the partial inversion of the A as a valid process.

Keynes of course does not finally retain this inference or any other in the regular table of immediate inferences where a particular conclusion is drawn from a universal premise. The rejection of all these is required by his particular theory of the existential import of categoricals, but the special discussion of the partial inverse of the A arises while he is provisionally assuming that "S, not-S, P, not-P, all represent existing classes". The "apparent anomaly" involved in passing from "All S are P" to "Some non-S are not P" is here taken to be explained by "the assumption of the existence of the contradictory of the original predicate", which he says may be expressed in the form "Some things are not P". He then notes that in this "additional premise" P is distributed.[5] This would mean, assuming we could take "Some things are not P" to state an O proposition, that the distributed character of the predicate of an O is essential to the partial inversion of an A. But also the inference would be some sort of mediate one in which the conclusion fills out the bare "things" in the alleged O premise with the opposite of the subject in the A premise. This is obviously a makeshift position. If the existence of the opposite of the original predicate (what Keynes calls its "contradictory") is especially noteworthy in connection with the partial inversion of an A, it is as something exemplified in the process as a whole, not as one premise entering into it. Keynes seems to take the existence of non-P in an extensive way, but it may be better to take it intensively as a matter of P having a definite intension. Such definite intension for P would be impossible if "All S are P" and "All non-S are P" could both be true, and so incidentally it would be impossible for P to be significant in either proposition.

Keynes's procedure here suggests that validity is identified with satisfying certain rules, and it is one of the important features of Anderson's syllogistic theory of distribution that it implies the direct recognition of the validity of various types of syllogism and of the invalidity of others. We may still, he argues, use the distribution rules in judging whether some syllogisms are valid or invalid, but this depends on our ability to recognise, apart from the use of rules, that certain types of syllogism are valid or invalid. Thus we could still sensibly say even that a particular AAA syllogism in figure II was invalid because it had no distribution of the middle term, but to recognise that the predicate of an A is undistributed we have to recognise directly that syllogisms of this type are invalid.

In the teaching of formal logic there may be a good deal to be said for giving in the first place a simpler non-syllogistic treatment of distribution statements and correcting this provisional treatment at a later stage. However, explanations are sometimes given that are so vague and so lacking in unity that a student might well learn no more than how to apply the words "distributed" and "undistributed" correctly. Thus of Whately, Mill and Keynes it is only the last who makes it at all clear that the question is throughout one of the status of a term *in a proposition*. From Whately and Mill one might easily imagine that a subject is distributed or undistributed in a secondary way in a proposition, and that the distinction is explicable by reference to such phrases as "All X" and "Some X" as though these had significance as units detached from the rest of the formulae "All X are Y" and "Some X are Y". On the other hand, in connection with predicates Whately is the most interesting of the three, since he tries to proceed here in terms of the predicate being affirmed or denied of the subject and does in a confused way indicate the possibility of valid substitution in respect of the predicate of a negative. He does not clarify matters when he says that in an affirmative proposition "a *part* only" of the predicate is affirmed of the subject, but the following is more promising: "If *any part* of the predicate is applicable to the subject, it may be affirmed, and, of course, cannot be denied of that subject; and consequently, when the predicate *is denied* of the subject, this implies that *no part* of that predicate is applicable to that subject; i.e. that the *whole* of the predicate is denied of the subject."[6] Keynes, however, speaks of our "excluding" the whole of P from the whole or a part of S in negative propositions, and Mill also in this connection uses language suited to specifying the relation between classes rather than the language of predication. If this were carried out in a systematic way it would not be possible to say with Mill that "in the proposition, All men are mortal, the subject, Man, is distributed, because mortality is affirmed of each and every man",[7] but the tendency is to adopt what Keynes calls a "predicative reading of the proposition" in respect of the distribution or lack of distribution of subjects and then to have some sort of class reading in respect of the status of predicate terms. These three writers give some general account of distribution and lack of distribution, but do not follow it up in a systematic way when they come to the more detailed exposition. There is not even a systematic treatment of subjects on the one hand and predicates on the other, except perhaps in Whately: the symmetry of the E and I propositions and the non-symmetry of the A and the O lead to complications.

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[5] Keynes: *Formal Logic*, 4th edition, pp. 139-40.

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[6] Whately: *Elements of Logic*, 9th edition, Bk. I, Section 5.
[7] Mill: *System of Logic*, 8th edition, Bk. I, ch. IV, Section 4.

III

The general explanations for terms being distributed or undistributed given by Keynes, Mill and Whately show only minor differences, and in all cases reference is made to the denotation of words so that the topic appears to be a verbal one. Keynes makes it fairly clear that the question is of a *proposition* giving information about the whole of what a term denotes or only about a part of this, but Whately and Mill in effect say that a term may be used to stand for everything which it denotes or to stand for only a part of this, being thus distributed or undistributed. Both these types of explanation encourage the view that the four forms of proposition traditionally recognised are reducible to aggregates or conjunctions of singular propositions, and Mill makes such a reduction in respect of universals when he is criticising syllogism as a mode of proof. In view of Mill's fairly forceful rejection both of a purely denotative treatment of concrete general names and also of class-interpretations of propositions, this reduction has always seemed curious to me, but his remarks on distribution make it somewhat more intelligible.

According to Mill "When a general name stands for each and every individual which it is a name of, or in other words, which it denotes, it is said by logicians to be *distributed,* or taken distributively".[8] I do not see how, in terms of Mill's theory of denotation, a general name could do otherwise than stand for every part of its denotation, but the distinction suggested here between a distributive and non-distributive taking of a general name seems to be important for the reduction of universal propositions. If Mill makes a distinction between a word's denoting various individuals and its standing for various individuals, then he might take the former to be governed by the connotation of a name (when the name is concrete and general) and not to vary with the different uses of the name while finding variation in the latter, a variation accompanying a variation in the use of the word. In this way he might attempt to justify the different treatments he gives of general names and general propositions in respect of reducibility.

There is in Mill no systematic development along reductionist lines of this notion of a special distributive use of general names; e.g. I do not think he ever suggests that a universal negative is a formula for a number of propositions which have individual *predicates* as well as individual subjects. But the reduction of universals to a number of propositions with singular subjects may well be connected with the notion. At one point in criticising syllogism as a mode of proof Mill definitely works to the force of a universal from the alleged force of such a phrase as "All men", and I think he would take the word "men" to be distributed in that *phrase*.

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[8] Mill: *loc. cit.,* Bk. I, ch. IV, Section 4.

He says that "Whoever pronounces the words, All men are mortal, has affirmed that Socrates is mortal, though he may never have heard of Socrates; for since Socrates, whether known to be so or not, really is a man, he is included in the words, All men, and in every assertion of which they are the subject".[9] Here Mill treats universal quantity as though it belonged primarily to the subject instead of to the proposition as a whole or as though the whole phrase "All men" signified the subject term. He is in fact treating the phrase as though it had collective force, though he does not of course say that the statement could be re-expressed as one concerning the whole group of men. That would be to treat it as dealing with an individual, as being a particular proposition in his phrasing, and while such a subject could include Socrates (e.g. he would be included in the whole group of philosophers in 400 B.C.) such physical inclusion is not what Mill is interested in. The group could possess features not possessed by each member and a statement about it could not plausibly be treated as a formula for a number of statements about the individual members. Mill, then, is not treating "All men" in any *simple* collective way, but it is only if the phrase is taken collectively that it would be sensible to say that it includes Socrates whereas "Some men" does not.

Now Mill neither denies nor affirms that a particular in the traditional sense states a number of his particular or singular propositions, but there are very obvious objections to treating it as a conjunction or an aggregate of singulars and I assume that

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[9] Mill: *loc. cit.*, Bk. II, ch. III, footnote to Section 8. This lengthy footnote dealing with criticisms by an unnamed reviewer and by de Morgan contains some interesting material relating to the criticism of syllogism considered as a mode of proof. It tries to bring this criticism together with the attack in ch. II on the *dictum de omni et nullo* and with the dictum, in terms of marks, which Mill there substituted for it. In my opinion the two sections fall apart and for consistency with the earlier material Mill should be criticising, as attempts at proof, only syllogisms interpreted in a certain way, i.e. as dealing with classes wrongly understood. However, there is no simple way of tidying up Mill's position along these lines. Sometimes in chapter III he emphasizes that he is concerned with an inconsistency in the logical theory of syllogism, but it is clear that he rejects syllogism as a mode of proof and not just some theory of the way in which it is a proof. He takes the major already to assert the conclusion even though within this footnote he says of the major that "the only indication it gives is a description by marks" so that "we have still to compare any new individual with the marks; and to show that this comparison has been made, is the office of the minor".

Mill would not so treat it. It is plain that we cannot in this case detach the sign of quantity from the proposition as a whole, unite it with the subject and obtain some set of individuals (less than those denoted by the subject term), which could function in an aggregate of singular propositions that would be equal to the original statement. Conceivably we might reduce a particular to another with a disjunctive subject including each individual which the original subject expression denotes, but there would be no way of deciding which of these individuals are to function as subjects in the aggregate or conjunction of propositions which is to be equivalent to the particular proposition.

Nevertheless, if anything in a universal statement could provide a multiplicity of individual subjects, it would be just the general subject expression which in other statements is accompanied by the sign of particularity. To get a coherent position in accordance with his reduction of universals Mill ought to follow the line which Keynes[10] attributes to Johnson, i.e. he should reduce a particular to the same singular propositions as the corresponding universal is reduced to, but take these to be disjoined in the first case while conjoined in the second. If we could once reduce a universal and a particular having a general subject to propositions with a disjunctive subject made up of individuals, it would be possible to obtain from the first a conjunction of singular propositions and from the second a disjunction of them, since we can argue from "All (A or B) are C" to "All A are C" and "All B are C" and from "Some (A or B) are C" to "Either Some A are C or Some B are C". However, Mill would not accept the first step here; he would reject the treatment of a general name that is involved.

That is made clear in the discussion of "perfect induction", where Mill insists again on the connotation of general names and dismisses the conclusions of these "inductions", which seem the statements most favourable to his view of universals, as "not really general propositions". Such a conclusion is described as "a mere short-hand registration of facts known",[11] whereas a universal, though it is "but an aggregate of particular truths", is "not merely a compendious form for recording and preserving in the memory of a number of particular facts, all of which have been observed".[12] Critics of Mill on syllogism sometimes neglect his rejection of "perfect induction", proceeding as though he *says* that every general proposition is arrived at by this process. Mill no doubt ought, in consistency, to say this, but whole sections of his chapter on syllogism would be ridiculous if he did and the denial of it is an important part of his position. It means that he does not depart in too flagrant a way from his early theory of general names, where denotation was subordinated to connotation. At the same time the connotative theory leaves it a mystery how "All men" contains Socrates, and it is of interest that in his *Examination of Sir William Hamilton's Philosophy* Mill says that " 'All men' and 'the class man' are expressions which point to nothing but attributes; they cannot be interpreted except in comprehension".[13] The treatment of general statements as asserting a number of singular propositions seems to be an excrescence in Mill's theory if we take his view of general names as central to it, though we could link this treatment with weaknesses in the latter view. An important thing here is that Mill tries to work to the meaning of propositions from the meaning of names. Presumably he would include the word "all" amongst those words which he says are not names but only parts of names, and in the chapter on syllogism his treatment of the word amounts to giving it *material* significance in the universal statement, even though it is not supposed to be functioning there in any simple collective way.

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[10] Keynes: *loc cit.*, pp. 479-80. Keynes speaks of the words *all* and *some* as "abbreviations of conjunctive and alternative synthesis respectively", but he does not explain how *other* words could be treated as abbreviations for the subjects of the propositions that are to be conjoined or disjoined.

[11] Mill: *loc. cit.*, Bk. III, ch. II, Section 1.

I do not wish to contend that Mill's reduction of universal statements stems from his views about distribution, though I take it to be required by them. In the *Logic* he gives only a very brief treatment of distribution in connection with the distinction commonly made between universal and particular propositions, and he does not discuss it in connection with the import of propositions. In the chapter on the latter topic he rejects the class-interpretation of propositions and offers a connotative one. But the *Examination of Sir William Hamilton's Philosophy* shows some concern with distribution when he takes the distribution rules of syllogism to be linked with "propositions in Extension" rather than with "judgments in Comprehension". He says that the framers of the rules "did not concern themselves with propositions or reasonings as they exist in thought but only as they are expressed in language".[14] It is not clear, though, whether he adopts an exclusively comprehension interpretation of propositions and takes distribution rules to be useful simply in connection with language, or whether he admits a secondary class interpretation with which they could be linked.

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[12] Mill: *loc. cit.*, Bk. II, ch. III, Section 3.

[13] Chapter XVIII, 6th edition.

[14] Chapter XXII.

IV

Whatever the rationale of Mill's thinking may be, some reduction of universal propositions, and of particulars as well, is encouraged by many explanations of distribution and of the lack of it. It is, e.g. encouraged by Keynes's material, although he might urge against this suggestion that he has upheld the "fourfold implication of propositions in connotation and denotation", so that an extensive reduction involving individuals would be no more possible than an intensive one which would presumably involve final descriptions. Keynes's readings in comprehension are the most relevant here, since comprehension is his objective intension corresponding to denotation as objective extension, while connotation is conventional intension. Taking the statement "All S are P", the possibility of reading this as "The comprehension of S includes that of P" and also as "The objects denoted by S are included within the class of objects denoted by P" might be said to rule out the reduction of it in either direction. There is no doubt, however, that talk of "reading" propositions in extension or in intension or in some mixed way suggests that the proposition gives information not about some single subject and predicate but about a whole variety of terms, even if it would be impossible to identify these in many cases.

No definite theory emerges from Keynes's discussion of propositions in extension and in intension. He speaks of relations between objects, relations between attributes and relations between objects and attributes involving one another so that "all of them are implied in any categorical proposition whose subject and predicate are both general names". He continues: "If any one of the relations is selected as constituting the *meaning* of the proposition, the other relations are at any rate involved as *implications*".[15] But once the possibility of just one type of reading is denied, the logical step is to drop readings altogether and to deal in some other way with any positive material that has been covered. Keynes sees that partial coincidence of the comprehensions of the subject and predicate is not conveyed by an I, and that the lack of such coincidence is not conveyed by the E. This means for him that there is no simple comprehensive reading of these corresponding to the class reading which he recognises, and so he turns to their obverses for a reading in comprehension. Thus "Some S are P" is read as "The comprehension of S does not include the comprehension of *not-P*", and strictly speaking a reading in comprehension is provided only for A and O propositions. Even there the reference to inclusion is unsatisfactory, as it is also in the class readings. Keynes is not using "include" in the sense required for inclusion being a relation on a par with intersection, exclusion, etc., and it would be better to set out the five extensive and the five intensive relations that are possible between two terms and to indicate what propositions involving these terms or their opposites mark off each relation.

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[15] Keynes: *loc. cit.*, ch. VI in Part II. Keynes here refers again to Distribution in support of a class reading, saying that "the predicate of a proposition must be read in denotation in order to give a meaning to the question whether it is distributed or not" (p. 181).

One aim of this note is to get away from the whole notion of "readings of a proposition", especially in connection with distribution. This notion might however be suggested by distribution statements even when these were not couched in terms of the denotation of names. Thus the rough and ready explanation offered in part I might suggest it. To say that a certain proposition applies to a term in its whole extension would readily suggest that the proposition at least is not being taken objectively but rather as a verbal formula which gives us information, not about the term to whose extension the proposition is said to apply, but about the various parts of that term's extension. But if the distribution statements are interpreted as Anderson interprets them, i.e. in terms of syllogisms in which the "applying" proposition is just one premise, we do away with any such suggestion. At the same time it would still be possible to link distribution with logical form,[16] just as we can link theory of opposition with that. The theory of distribution would not become simply a part of the theory of syllogism and it would still be sensible to say that we do not understand a formula such as "All men are mortal" until we realise that it conveys a proposition in which the subject is distributed and the predicate is not. But now understanding it would not be treated as analysing it in the fashion of Mill into a distinct set of statements. Again it would not be necessary, in order to understand the statement, to adopt a class-reading of it either as its "meaning" or as an "implication" of it.

Comparing Anderson's position with that of Keynes it might be suggested that the former requires a propositional "reading" of class-relations rather than a class reading of propositions, and the valid syllogisms to be specified later might be relevant to such a reading of the relations. However, I do not wish to discuss that here. The main point about Anderson's logic relevant to my topic is that questions of meaning do not confront us at the outset.

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[16] Compare Anderson's remarks in "Hypotheticals" (Vol. XXX, No. 1 of this *Journal*) on the "capacity" of the A proposition for being a major in a number of Barbara syllogisms.

In particular we are not confronted with some theory of the denotation and connotation of names (or of their extension and intension, to use Keynes's more general expressions) from which a theory of the import of statements is derived. Instead, starting from statements as signifying propositions, a formal theory of terms is worked out, including the theory of their extensive and intensive relations.

V

Anderson's formal treatment of distribution statements may be approached via the rough formula that a term is distributed in a proposition if and only if that proposition applies to it in its whole extension. Understanding that the applying is not a matter of a set of words meaning something or other, this formula leads to a view of distribution in terms of implication. It indicates that a proposition may or may not apply to the whole of the extension of one of its terms. It may or may not then justify the results obtained when we "apply" a proposition to various parts of the extension of a term in it, when in other words we make extensive substitutions in respect of one of its terms. Strictly speaking, these results are never justified by a single proposition, for any process of substitution involves three propositions. It involves the information that a certain term is part of the extension of a term in the given proposition, and to know that one term is part of the extension of another is to know that it is the subject of a true A proposition which has that second term as predicate.[17] Since such an A proposition enters into any process of extensive substitution, we may distinguish the two types by referring just to the original proposition, but strictly the question is always whether a pair of syllogistic premises justifies or implies the result. When it does the term in respect of which the substitution was made is distributed in the given proposition, and when it does not it is undistributed there.

It is possible and useful for some purposes to put the distinction by reference to there being or not being loss of truth in a process of extensive substitution. This will not, however, be adequate to the distinction between the distribution and the lack of distribution of a term in a proposition unless it is understood that when a term is distributed in a given proposition (e.g. the term "human" in the proposition "All human are organic") there is no loss of truth in extensive substitutions either in respect of this term in this specific proposition or in respect of any other term which is functioning in the same way as the subject of an A proposition. It is easy to find an I or O proposition such that extensive substitution in respect of its subject always leads to true results. This must be so with any particular which is the subaltern of a true universal. And while extensive substitution in respect of the predicate of an A will lead to some false results, no matter what specific A proposition we take, the position with regard to the predicate of an I is similar to that holding with regard to the subject. Here again, by calling in syllogism and immediate inference, we see that in respect of certain I propositions the substitution process will always give true results. This must be so when the I is the converse of a true A proposition, but it would mean the end of any formal notion of distribution to say that the term human is distributed in "Some organic are human" or again in "Some human are organic".

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[17] This "formal" treatment of extension, which does not take individuals as the sole or as the ultimate parts of an extension, is an important element in Anderson's general view.

On Anderson's view the distribution of the subject of an A appears as a matter of the validity of AAA syllogisms in the first figure. In such a syllogism the minor premise specifies an extensive value for the middle term and the validity of the syllogism shows that the middle term is distributed as the subject of the major premise. Similarly, to say that the subject of an E is distributed is to say that EAE in the first figure is valid. Then the distribution of predicates takes us to figure II syllogisms. In a proposition "Some X are not Y" the predicate is distributed, since the knowledge that "All P are Y" permits us to substitute P for Y in "Some X are not Y", i.e. permits us to proceed to the proposition "Some X are not P". This means that AOO in figure II is valid, and, equally, to say that the predicate of an E is distributed is to say that AEE in figure II is valid. Then to say that the predicate of an A proposition is undistributed is to say that AAA in figure II is an invalid syllogism. This is the syllogism which may be viewed as substituting an extensive value of the middle term for that term as the predicate of an A (i.e. in the minor premise), the value being obtained from the major premise. Similiarly, AII in figure II is invalid and this is what we mean by saying that the predicate of an I is undistributed. That the subject of an I is undistributed is a matter of IAI in the first figure being invalid, and that the subject of an O is undistributed is a matter of OAO in that figure being invalid.

I wish to say also that the validity of AAA in the first figure means that the predicate of an A is significant. Here we should have to think of the minor premise as the one in which a substitution is made, a substitution namely of an intensive value of the middle term obtained from the major. A syllogism of this type indicates that the middle term is significant in the minor premise.

It is not of course significant in the major premise, where it is the subject of an A, and that is seen by reference to the invalidity of AAA in figure III. Similarly the validity of AII in the first figure means that the predicate of an I is significant and the validity of IAI in figure III means that the subject of an I is significant. Again, the subject of an O is significant, since OAO in figure III is valid, while its predicate is not significant, since AOO in the first figure is invalid. Finally, neither term is significant in an E since EAE in figure III is invalid (the subject is not significant) and AEE in the first figure is also invalid (the predicate is not significant). In each of the syllogisms mentioned here there is an A premise which gives an intensive value for the middle term; it is understood throughout that a term is part of the intension of another if and only if it is the predicate of a true A proposition in which that other term is the subject.

It is of interest that AII in the first figure belongs to the set of syllogisms relating to significance and not to those relevant to distribution. Of course the proposition which is its major premise does apply to its subject in its whole extension, but the syllogism does nothing to show that. This is the mood to which OAO in figure III is most simply reduced, and it conflicts with the treatment of syllogism as always extensive substitution, just as that mood does. The major premise gives an intensive value for the middle term but neither premise gives an extensive value for this term. This mood then does not come immediately under a dictum that "whatever is predicated (i.e. affirmed or denied) universally of any Class of things, may be predicated, in like manner, (viz. affirmed or denied), of anything comprehended in that Class",[18] unless of course "comprehended in" a class just means not excluded from it and so is compatible with the I as well as with the A minor premise. The two 'particular' moods of the first figure (those with a particular premise, namely AII and EIO) are not immediately covered by the dictum if this requires that the minor is itself an instance of the middle term. They can, as Aristotle points out, be reduced in a roundabout way to the 'universal' moods AAA and EAE, but their difference from these moods should not be neglected. When the I minor premise is true, it must be possible to replace the minor term by another term to which the middle term belongs universally (or which is an instance of the middle term), and then we could draw an A conclusion instead of the I and an E conclusion instead of the O, but this does not mean that AII can be run together with AAA or that EIO can be run together with EAE in an attempt to present all the valid moods of the first figure as cases of application. The two moods that have weakened conclusions may properly be described as cases of application, but not the two with a particular premise.

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[18] Whately: *loc. cit.*, Bk. I, Section 4.

If a dictum is wanted which is connected with application, as Bain suggested in criticism of Mill's correction of the old dictum, the best procedure is surely to derive it simply from the AAA syllogism in the first figure. If we confine ourselves to the first figure we are already neglecting some cases of application, namely those syllogisms in figure II where one premise applies to the subject of the other in respect of its own predicate, and concentration on AAA within the first figure will give us the simplest possible dictum connected with application. Keeping fairly close to the old dictum we might say that "Every predicate of a term is a predicate of anything of which that term is a predicate". This emphasizes the major term, which is the "predicate of a term" spoken of, and there is a simple contrast between it and another dictum, also derivable from AAA and comparable to Mill's corrected dictum, which emphasizes the minor term. Mill offered "Whatever is a mark of any mark, is a mark of that which this last is a mark of",[19] and he is using "mark" in such a way that this amounts to saying that "Every subject of a term that is itself a subject of something else is also a subject of that further term". The "subject of a term" spoken of here is the minor term, and this dictum may be said to point to the fact that AAA may be regarded as an intensive substitution of the major term for the middle term as this appears in the minor premise. The older dictum, with its emphasis on the major term, points to the other way of regarding the AAA syllogism, as an extensive substitution of the minor term for the middle term as this appears in the major premise.

The older dictum was meant to cover all the valid moods of the first figure, and it could, no doubt, be reworded so that it would do this without suggesting that these moods are all cases of application. But it is quite useful to have emphasis on application in a dictum so long as it is realised that another dictum is possible in which the emphasis is on significance. A dictum which reminds us that the subject of an A is distributed may seem more natural than one reminding us that the predicate of an A is significant, but when we have the two before us it becomes clear that each is only a way of presenting a valid syllogistic form, one that is convenient to use to show the validity of all other valid types of syllogism.

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[19] Mill: *System of Logic,* Bk. II, ch. II, Section 4.

PURE BECOMING

By Richard Taylor

Being and becoming

In order for anything to be this or that—for instance, red, square, large or whatnot—it must first of all *be*; not "first of all" in time, certainly, for it might always have had just the properties it has, but first in the sense of logical priority. But while a thing's having any particular property entails that it is, and its having being entails that it has some property or other, yet there is no property which anything must have, in order to be.[1]

This sort of consideration (with others) long ago led philosophers to speak of *pure being*, as contrasted with being this or that, and of substance as contrasted with properties and accidents, while the priority or "firstness" of existence over the possession of properties begat such kindred notions as *primary* being. Exasperating opinions yielded by this concept, especially as applied by theologians, have also stimulated philosophers to invent arguments to show that existence is no real predicate—a conclusion which, however, obviously does not affect the evident truth with which we began, *viz.,* that in order to be this or that, a thing must first be, or exist.

Now it has apparently hardly been noticed, if noticed at all, that a similar distinction can be drawn in the concept of *becoming.* Thus, in order for anything to become this or that—e.g., to become red, square or large—it must first of all *become*; that is, essentially, it must *endure* or, in an odd and special sense, *become older*; not "first" in the temporal sense, obviously, but again in the sense of logical priority. Thus, if anything becomes red, it also becomes older, and while the fact that it becomes anything at all entails that it becomes older, this latter fact, the mere fact of its enduring, does not entail that it becomes anything else whatever. Let us, then, distinguish the notion of *pure becoming,* which is simply that kind of becoming which is presupposed by a thing's becoming anything whatever, but which, unlike any other change, presupposes no other becoming at all. And here we may note, incidentally, a respect in which pure becoming is not analogous to pure being; namely, that while there is perhaps no property that something must have in order to be, it is nevertheless impossible that anything might exist without having *some* property; but in the case of pure becoming, it is quite possible that something might simply become, without becoming anything at all, except older.

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[1] Unless it is some queer property, like the property of having some property.

Pure being, in other words, could not possibly have an instance, and can only be a distinction of reason, whereas pure becoming, as I have described it, could have an instance, though it doubtless does not. It is possible to imagine a stone, for instance, that had no history over a period of time, excepting *only* that it became older.

Whatever one might think of pure being, and whether existence is a real though manifestly peculiar state—about which I shall say no more—it is worth while considering pure becoming, and asking whether this is a real though manifestly peculiar kind of change. I shall, then, present a case for this notion, elicit a few of the odd statements it permits us to make, and then see whether there is any final way to remove their oddness.

Pure becoming

Concerning anything there is,[2] we can assert at least one thing, other than that it is, namely, that it is becoming older—and this we can do, without looking to see. Now it can, if we like, be as stable and unchanging as we care to suppose, but this is one change it cannot elude. Indeed, even though it should acquire the attributes of newness, it still becomes older in doing so. A flower, for instance, which resumed the form of a bud, would meanwhile grow older, for otherwise no meaning could be given to its *resuming* this form. We can say of it today, whatever may be its form, that it is a day older than it was yesterday, which entails that it has *become* older, however youthful it might appear. And this, certainly, is a change, though not an observable one; what was the case, that it had an age of so many days, is no longer the case, for it has in the meantime acquired a larger age.[3]

Now anything, to become older, must of course first *be*. Nonentities do not endure, nor do real things which have ceased to be, or which will be but are not yet real. Diogenes' cup became old and rusty, but both changes ceased with the cessation of the cup itself—otherwise, we should be able to predicate of it mutually incompatible total ages. Yet even things which have ceased to be, and others which are not yet, undergo a *relational* change which is but a corollary of pure becoming. That is, we can speak of Diogenes' cup as receding ever farther into the past; it is more remote from us today than it was yesterday, and this is a (relational) change which it is undergoing. Similarly, the birth of my first grandchild, assuming there is to be one, is something that is drawing closer; I can look forward to it now, but some day I shall only be able to look back on it; and this is a change which *it* is undergoing, with respect to me, and not merely a change in the manner in which I choose to look upon it.

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[2] Except, of course, eternal things, if there are any such.

[3] A similar point is made by A. N. Prior, "Time After Time", *Mind*, Vol. 67 (1958), p. 244.

Moreover—and strangest of all—times themselves seem to undergo both kinds of change just described, any actual time, like any actual being, undergoing the same pure becoming as the latter, and any non-actual past or future time undergoing the relational change characteristic of all non-actual past or future beings. Thus, I can say of *today* that most of it has elapsed, and its end is drawing closer—in short, that even *it* is becoming older; what was true of it this morning, is no longer true of it this evening, and that is no less a change of the day itself, i.e., a change of age, than of any being or process occupying that day. Nor does this just express the trivial truth that at, say, 9:00 o'clock, today is nine hours old, and at 10:00 o'clock an hour older; for this statement is true of days that are not becoming older—of yesterday and tomorrow, for instance.

Similarly, just as Diogenes' cup is receding into the past, and the birth of my first grandchild drawing ever closer, so also are the *times* of these doing the same. The time that Diogenes took his last sip from his cup is, like that last sip itself, more remote from us today than it was yesterday, and less so than it will become tomorrow; and this is a relational change, with respect to us, of that time itself.

The peculiarities of this

This kind of change is admittedly very odd, and appears even more so in the light of the following considerations. First, that not only is pure becoming an unobservable change, it is compatible with, and even entailed by, any change whatever that *is* observed; it is, then, a purely *a priori* notion. Second, any event is a change, and it is extraordinary then to speak of events themselves as changing. Yet any event, such as a battle, does change in this way—while it lasts, it becomes older, and then begins an endless recession into the past. Third, all ordinary changes presuppose time, and it is extraordinary that there could be any kind of change that times themselves might undergo, unless we are willing to postulate a mysterious kind of "time" against the background of which the ordinary time intervals, into which we are born and grow older, can themselves come into being and age.[4] And finally, the continual recession of things past, and the advance upon us of things future, is peculiar in that, unlike other changes, no *rate* can possibly be assigned to it, without making nonsense. It clearly does not do, for instance, to say that Diogenes' cup is receding into the past at the rate of one day every twenty-four hours; but what else can be said?[5]

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[4] It is nonsense to speak of *time* coming into being, but not obvious nonsense to speak thus of intervals of time, such as days and years.

Yet we cannot simply *pronounce* pure becoming to be an illegitimate concept, just because of its manifest peculiarity and puzzlingness, unless we are very lazy scholars. Statements embodying this concept are often made and readily understood. They obviously, then, express facts of *some* sort, and sometimes very important ones—e.g., "the hour of your death is approaching and near", uttered to someone awaiting execution. What is needed, then, is some *other* way of expressing those same facts, getting rid of what seem to be odd implications, without also destroying the truth that is in them.

And one can indeed, as I want now to show, find another way of expressing *any* such statement; but—this is the hitch—not *all* such statements can be reformulated, for any reformulation will presuppose at least *one* statement embodying the notion of pure becoming to be irreducibly correct as it stands. Our position is, I believe, like that of a man who can easily get rid of mice, if he is willing to put up with cats, or can get rid of the cats, if only he can tolerate mice, but who cannot be rid of *both* cats and mice.

The attempt to expurgate pure becoming

Our problem will be to try to find some way of restating all of the following four sentences, wherein *X* designates an object or event and *Y* a time or interval of time:

(1) *X* is growing older.
(2) *X* is receding even farther into the past.
(3) *Y* is growing older.
(4) *Y* is receding even farther into the past.

If we could reformulate just these four, so as to convey such facts as they may express, but without tacitly employing the notion of pure becoming, then there would not, I think, be any statement of the kind elicited in this discussion which could not be similarly restated.

Consider, then, our first sentence, "*X* is growing older". This might seem equivalent to "*X* occupies an interval of time",

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[5] A. N. Prior (*op. cit.*) maintains that this way of assigning a "rate" to such change is not senseless, but I submit that it is.

"*X* exists for more than an instant", "*X* endures", or, to get rid of the verb, simply "*X* has duration".

These expressions are *not* equivalent to our original, however, for any of them might apply to something that is not becoming older—to all past things, for example, that lasted more than an instant, like Diogenes' cup. This cup, we noted, ceased growing older as soon as it ceased to be—yet it would be false to deny of it existence throughout an interval of time.

What obviously needs to be done, then, to establish an equivalence, is to construe such words as "occupies", "endures", and "exists" as conveying a present tense. In fact this can be made quite explicit, either by qualifying each of these statements with "now"—for instance, "*X now* occupies an interval of time"—or by saying, more elaborately, "*X* occupies an interval of time, and that interval has not yet reached its termination". But as soon as we have done this, we have obviously reintroduced pure becoming, merely transferring it from *X* to some elapsing time. Or, to make the same point, we can say of this time, this "now" that must be brought in, *either* (1) that *it* is drawing to an end, or growing older, in case "now" is taken to mean an elapsing interval, such as a day, which can more than once be called "now"; *or* (2) that *it* recedes ever farther into the past, in case "now" is taken to refer to some particular *instant* of time which ceases to be as soon as it is mentioned; *or* (3) that all past things are receding in relation to *it,* in case "now" is understood as continuously referring to some *new* instant each time it is used.

Similar difficulties confront us if we try to reformulate our second sentence, "*X* is receding ever farther into the past". This is not equivalent simply to "*X* is in time", for future things are in time too; nor to "*X* exists in the past", for many things, such as myself, that exist in the past have continued their existence into the future, and it would not be true to say that I, for instance, am receding into the past. Nor does it help to say "*X* has its *whole* existence in the past", or to give some name to that past interval through which it endured, such as "the year 1957", because, as we have seen, we can say of that interval, no less than of *X,* that *it* is receding into the past, the gap between us and that interval growing larger just as certainly as the gap between us and *X,* it being, indeed, the same gap. It would seem, in fact, that the notion of this recession is an ingredient in our very conception of the *past,* in which case there is little hope of getting rid of the one by appealing to the other. We have again, then, not expurgated pure becoming from our thinking, but simply transferred it from *X* to some *Y,* which is the time of *X*.

I believe a similar result will come from *any* attempt to reformulate our first two sentences, but this need not be discouraging, for we can apparently always get rid of the pure becoming of things and events so long as we can ascribe it to times; and if we can now get rid of the pure becoming of times, we shall have found a way to rid ourselves of it altogether.

Let us, then, consider our third sentence, "*Y* is becoming older", which ascribes pure becoming to some interval of time—to a day, let us suppose. This is plainly not equivalent to saying merely that *Y is* an interval of time, such as a day, for not every such interval is becoming older. The day of Diogenes' birth, for example, is a day, but not one that is elapsing or drawing to an end. Plainly, if *Y* is a day, then of all the days one might name, only one of them can be *Y*, namely, today. Is our sentence, then, if we allow *Y* to stand for some day, simply equivalent to "*Y* is today"? It is, but this does not get us far, because the *only* thing that distinguishes this day from all the others is that it is the only one that is drawing to a close, or growing older, so the identification of *Y* with today does not rid us of that notion. One might try to identify this day in some other manner, e.g., by saying that *Y* is the day on which *X* occurs, but that does not work, because it is always a synthetic proposition that some *X* occurs today; "*Y* is the day on which *X* occurs" might then be true, and "*Y* is becoming older" might be false—indeed, *must* sometimes be false. Nor will it help to identify *Y* by its date, for it is similarly a synthetic proposition that today falls on that date—in being told merely that *Y* is identical with this or that date, I do not know that today is *Y*. If, further, our original sentence is reformulated as "*Y* is an interval of which an instantaneous *now* is a part", then we only shift the becoming of this interval to the *now* which is a part of it, for we can say of this *now*, either (1) that the beginning of *Y* is receding in relation to it, if "now" is understood as being capable of referring to more than one time, or (2) that this *now* will become a part of a day which is not *Y*, or (3) that this *now* is itself receding into the past, if "now" is construed as designating only *one* time—and all these statements embody the notion of pure becoming. Nor will it help, of course, to identify *Y* as the day of *this utterance* (or this whatnot), for we shall need to know that by *this* utterance is meant one occurring *now* (as it might not be, in the case of utterances written or recorded), and we therefore do not get rid of the pure becoming of *now* by resorting to a *this*.

Of course similar difficulties will arise in trying to reformulate our last sentence, "*Y* is receding ever farther into the past", wherein *Y* designates an instant or an interval of past time. We cannot just say that *Y* is the time at which some past *X* happens, for we were able to get rid of the recession of this *X* only by transferring it to a time, and this reformulation would only put us back where we started. We might, then, say that *Y* is entirely a past time—but this does not help either, for we can immediately add that its pastness is something which it *became,* and that it is now becoming *increasingly* past, there being no past time which has not also been future. The idea of becoming, in short, is part of the very notion of anything's being past, and there is no hope of being rid of that idea by resorting to the concept of pastness.

No other relation, however, will work at all. We cannot just say that *Y* is *earlier* or *anterior* to some time, for instance, for this is true of all times whatever, including those which are not receding into the past, namely, all future times. We must identify the time to which *Y* is anterior, either as being the present time, or some time itself anterior to the present, and the hopelessness of this, as a means of avoiding pure becoming, is quite obvious. For we can note that the time we are calling "the present time" either has not always been present, but became such—in which case, though *Y* has indeed always been anterior to it, it is a time which undergoes the same pure becoming as *Y*—or, that if in some sense "the present" time *has* always been the present time (this having the appearance of a logical and hence eternal truth), and hence never became such, then plainly *Y* has not always been anterior to it, but *became* such.

Conclusion

I think a similar failure will result from any attempt to reformulate our last two sentences, and any others significantly like them, such as "summer will soon be upon us", or "I am reaching the age of retirement". One seems justified in concluding, therefore, that the pure becoming of temporal relations is no myth or metaphor, and that this strange notion cannot be purged from our understanding of temporal relations without destroying our very understanding of those relations themselves, as temporal.

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CONFUSIONS IN CONTEMPORARY LINGUISTICS

By R. George Overend

In an earlier article I pointed out[1] that contemporary philosophic analysis is not philosophy and mentioned briefly that this is sometimes obscured by its being written elliptically. Since I think this latter fact removes a lot of mysteries from recent thought, we should perhaps discuss it in greater detail.

No one doubts there is a novel movement afoot in philosophy —my point is that it is only novel in ignoring factual problems, in confining itself to what I have called "pure analysis", by which I mean things like: "Naïve realism leads to physics, and physics, if true, shows that naïve realism is false. Therefore, naïve realism, if true, is false; therefore it is false." This is a series of "if-thens", deals only with the intrinsic consistency of statements, has no necessary reference to fact. So, for example, naïve realism *as a theory* could be self-contradictory without showing that perception itself does not *in fact* occur that way. The analysis of perceptual or other philosophic theories should then be regarded as meta-philosophy just as the analysis of science is meta-science. Woodger, in biology, prefers to talk of a distinction between language and meta-language, but he means the same thing. And in physics we might, say, examine the physicist's statement that "there is no perfectly accurate method known for measuring temperature" together with what he means by "temperature" throughout his work and might conclude that his statement is really figurative; our argument might run as follows: In any heat theory the term "temperature" is not used for any vague or general notion but for "temperature as measured by a certain change in a certain physical property". The physicist chooses the property and, once chosen, all his statements referring to temperature can be exactly translated into statements referring only to this property: no other sense of "temperature" is involved once the choice is made. So instead of talking of measurement he should talk of operational definition, and no question of inaccuracy arises—for that at least two senses of the term are needed. And that is how we might argue; right or wrong, it illustrates the distinction I am emphasizing between pure analysis or meta-theory on the one hand and theory or description on the other.

Now we have our terms straight, I want to point out that, apart from all the unambiguous pure analysis in contemporary philosophy, many things (like *Language, Truth and Logic*) are confusing because they are written in an epistemological idiom.

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[1] "A Note on the Meaning of Contemporary Logic", this *Journal*, August 1958.

But worse, "philosophical analysis" has become such a fashionable title that many talk and write factual, traditional philosophy in a pure-analysis idiom. Once we realize this the contemporary landscape becomes simple.

First some examples of people who know and say exactly what they are doing. Ernest Nagel's "Logic Without Ontology" merely shows that *a* meaning can be given "to the logico-mathematical disciplines without requiring the invention of a hypostatic subject matter for them". Most of Tarski's work is the same, what he calls "semantics": merely attempts to describe accurately the basic structural properties of a language, "a sober and modest discipline which", he himself says, "has no pretensions of being a universal patent medicine" but which nevertheless "plays an important role in the discussions of philosophers, logicians and philologists". It plays a role in philosophic inquiry but is not itself philosophy. An example is his "Semantic Conception of Truth", which tries to characterize "true" as it occurs in ordinary language, and he takes care to point out that "it implies nothing regarding the conditions under which a sentence like 'snow is white' can be asserted. It implies only that, whenever we assert or reject this sentence, we must be prepared to assert or reject the correlated sentence, 'the sentence "snow is white" is true'. Thus we may accept this semantic conception of truth without giving up any epistemological attitude we may have had; we may remain naïve realists, critical realists, empiricists or metaphysicians—whatever we were before. The semantic conception is completely neutral towards all these issues."

Then Ayer's "Negation" is fairly unambiguous, sets out, without committing himself to any factual doctrine in particular, to find what could be meant by a negative statement *in ordinary usage* and suggests that, as used, "x is not blue" is equivalent to "x is eulb", where "eulb" stands for possible qualities in what he calls a range. Comparison of this with what traditional logicians said on negative judgements[2] will make the self-imposed restrictions of Ayer's paper clear. Or take the following argument from Ryle's "Systematically Misleading Expressions": "Suppose I assert of (apparently) the general subject 'carnivorous cows' that they 'do not exist', and my assertion is true, I cannot really be talking about carnivorous cows, for there are none. So it follows that the expression 'carnivorous cows' is not really being used, though the grammatical appearances are to the contrary, to denote the thing or things of which the predicate is being asserted. And in the same way, as the verb 'exists' is not signifying the character asserted, although grammatically it looks as if it was, the real predicate must be looked for elsewhere. . . . So 'carnivorous cows do not exist' ought to be rendered 'nothing is both a cow and carnivorous', which does not as it stands imply that anything is either." That is perfect pure-analysis and we are left with no doubt about it: it has no direct relevance to cows or their existence though it does say that certain *descriptions* are, *in their own terms,* of mixed meaning. Likewise, "Categories" is fairly unambiguous, making it clear that "proposition-factors" (unlike the "categories" of traditional discussions) have no status in themselves.

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[2] Cf., for example, W. E. Johnson: *Logic*, Vol. 1, Ch. 5; or W. S. Jevons: *Principles of Science*, 2nd edition, pp. 44-5.

But not always has the meaning of these people been so obvious: pure analysis often comes in foreign wrappers. Some commentators, for instance, would say the earlier work of Ayer was fundamentally different from what I have been describing. But let us see. Pardon me if I repeat the central point of *Language, Truth and Logic*: Statements are either "analytic" (tautological) or "synthetic" (descriptive of fact) or "basic" ("referential" in some way to sense-data but too elementary to be informative) or rubbish. Statements purporting to be descriptive of fact are discriminated by the verification principle, a statement of yours being meaningful if you know how it could be verified, what observations would be relevant, otherwise neither true nor false but literally senseless. That might be well known, but what seems to have escaped notice is that the doctrine is derived very indirectly, that in fact it has not got outside language. For instance, he says the rationalist's claim to *a priori* truths is logically untenable; they must therefore, he says, be either inductive generalizations or tautologies—the only other possibilities. But they can be shown to be necessary, and this he does by examining cases in which they might seem to be confuted; therefore, he says, they must be tautological. Then he shows that mathematics functions just as well on these as on "axioms" and that formal logic gets on just as well without the "laws of thought". And once *a priori* truths are "eliminated", the right of metaphysics to build an "absolute explanation" of reality has also gone, for it will need some *a priori* "first principles" and there are none. And so the book proceeds very neatly from thesis to thesis, and it is all quite watertight—begin anywhere and you will be taken by implication right around. But to derive a positive doctrine from negative reasoning you need a closed system and this is something elimination itself cannot supply. So, somewhat admitting this, Ayer has to say that the verification principle (or the classification of statements—it doesn't matter which we take, for one entails the other) is itself a definition; but in his sense of definition this waters it down too much, so he adds that it is not entirely arbitrary, nowhere telling us exactly what this means. Only one thing remains certain, that whatever these theses are they are not factual but linguistic. And if they weren't this they would be metaphysical. But to the extent that this is true, the verification principle does not eliminate metaphysics in general: consistent analysis can only point to intrinsic absurdities and only by instance, that is, it cannot invoke any general principles of its own—as Ayer realized when he added in the 1946 Introduction that metaphysical theories must be examined individually. Which means, does it not, that the verification principle is superfluous? So the underlying doctrine of the book disappears and we are left with a somewhat random collection of examples in linguistic clarification, very useful (I am not underrating their importance) but not philosophy.

Similarly, *The Concept of Mind,* if it is not to annihilate itself at the very outset, cannot be read as anything but pure analysis. A *reductio ad absurdum* elimination of a number of traditional descriptions of the mind will, if the arguments are sound, make us dismiss these descriptions but not of necessity the mind. No, it is all indistinguishable from the less elliptical later Wittgenstein, and when Ayer talks empiricism and Ryle behaviourism (and they still do this) we must assume it is merely idiom. Either this or they are endowing language with some unusual powers of generating out of itself factual conclusions. But the work of an analyst, however else we characterize it, implies a distinction between language and fact, and factual conclusions will not then follow from linguistic analysis *unless* independent philosophic presuppositions are made about the nature of language in the first place. So rather than say they are writing sheer philosophic dogma (which is what it would be on their own sort of criterion) I am crediting them with not having written philosophy at all. Moreover, if I am wrong, what novelty constitutes what they (with others) call the revolution in philosophy? And why would Ryle then call *The Concept of Mind* a logical geography? And so we could go on. Of course, all I am saying about Ryle and Ayer here can be seen to apply to others as well.

It is also illustrated by J. J. C. Smart's discussion "Philosophy and Religion" (this *Journal,* May 1958). Smart has realized (from criticism by Passmore and Boyce Gibson) that you can't draw the ontological (i.e., factual) conclusion "God does not exist" from the linguistic confutation of any number of *theories* asserting that God exists. So he now wants to devise a new type of "linguistic" inquiry ("rational reconstruction"—already going in the United States) and he now wants to say also that *The*

Concept of Mind is this sort of thing. But what could such an inquiry be if not just factual philosophy—such bad philosophy in fact that Smart thinks he has dismissed God in one page? If scientific theory as a model of knowledge is not one of the things philosophers have been asserting or denying for centuries, what is? As Smart's opponents in his religious debate may well point out, all he is offering is a new factual idiom for his analysis.

We come, finally, to deal with the reverse, with a class of people who write factual theory in a pure-analysis idiom, these not therefore being "philosophical analysts" in *any* sense, but traditional philosophers. The simplest example is probably R. M. Hare's *Language of Morals*. Wanting, presumably, to be in fashion, he says it is "a logical study of the language of morals", but in fact it depends on philosophic presuppositions, brings in sociological observations and tries to tell us how we develop reasons for choosing actions, all this somehow translated into figurative talk about analysing language. The result is complex and quite unnecessary confusion.

I have already described Tarski's "The Semantic Conception of Truth" and Nagel's "Logic Without Ontology" as pure analysis. Feigl and Sellars have included both these in their *Readings in Philosophical Analysis*. But in the same book we find papers which, stripped of a little metaphor, are in no way different from traditional philosophy. Which shows that Feigl and Sellars are apparently deceived on this issue. Kasimur Ajdukiewicz tries ("The Scientific World Perspective") to prove the epistemological thesis that science gives only an "aspect picture" of the world, out of a number of possibles, and he gets there by: (1) the assumption (he says it's not an assumption, but dead obvious) that the description of the world that a scientific theory gives is a function of three things, (a) "picked out" raw experiences, (b) a superimposed conceptual framework, such being at least partially a matter of choice, and (c) language rules that "fix" the descriptive language to the concepts (the concepts are the meanings of the sentences); and (2) examples of factual evidence looked at in the light of (1). All of which is vastly different from Tarski's semantics or other pure analysis. And these are not the only such mysteries of the anthology: what is Schlick's "Meaning and Verification" if not epistemology? What semantic evidence (in Tarski's sense) would be sufficient to justify the assertion that the meaning of a proposition is the method of its verification, that "stating the meaning of a sentence amounts to stating the rules according to which the sentence is to be used"? A behaviouristic psychology of language? Croce, Collingwood and Susanne Langer (philosophers, not analysts) would form an opposition camp on this same issue, and how could anything but philosophy settle the dispute?

No wonder Feigl and Sellars do not define "philosophical analysis" for us in detail or tell us precisely in what way its genius is novel! And in the *Logic and Language* series selected by Antony Flew we find the same diversity of material. Things like Ryle's "Categories" go along with things like G. A. Paul's "Is There a Problem about Sense-data?"—which sets out to ask whether there are such things as sense-data and, if so, whether they are private, whether they can exist unsensed and whether they are momentary or continue to exist for a period. He does not answer the first question, but thinks he has said something definite on the others. And he would be forced to admit that this is not essentially different from the arguments of traditional philosophy or from the sort of work people like J. R. Smythies are doing in the philosophic implications of neurophysiological data.[3]

As I have said, purely linguistic evidence cannot settle these matters—and perhaps Flew's title *Essays in Conceptual Analysis* for his third series means he is beginning to agree. Nevertheless, in Britain at least, these articles still go under the name "philosophical analysis". John Hospers asks "What is Explanation?", says the answer is simple: "to explain an event is simply to bring it under a law . . . a true, that is, a really existing, uniformity of nature". The question behind this, which has been argued, once again, for centuries, is now supposed to be answered in a few pages—by use of a language idiom.

Then again we might be forgiven for thinking that Max Black has sorted out this difference between pure analysis and philosophy. *Language and Philosophy* opens with an ostensive definition of "linguistic analysis": using a certain sceptical doctrine as subject-matter, Black says "We are, it is true, not offering direct evidence for or against the sceptic's position . . . but we do invite him to reflect conscientiously and persistently upon the meanings of the terms he is using". But we find that "Vagueness" presents a thesis to which a collection of psychological data on the nature of language is essential.

So the contemporary landscape is cluttered up with a widespread traffic in fashionable idiom and when we remove all the metaphor we see that in some cases not much remains. In other words, it is time we dropped façades and got back to philosophic problems themselves, using analysis of language where it helps but not doting on it.

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[3] Cf., for example, J. R. Smythies: *The Analysis of Perception*, 1956.

DISCUSSION

SENSATIONS AND BRAIN PROCESSES: A REPLY TO PROFESSOR SMART

I. By George Pitcher

Professor J. J. C. Smart in his article "Sensations and Brain Processes"[1] defends against various objections the thesis that sensations are brain processes. He takes, as a typical example of having a sensation, the having or seeing of an after-image. This is an error: an after-image is not one sort of sensation, nor is the seeing of an after-image one sort of having a sensation.[2] But this mistake may be ignored: let us take his thesis to be the weaker one that the having of an after-image is a brain process. It is not the after-image itself which he holds to be a brain process, but the experience of having an after-image.[3]

Smart is not recommending that we drop the term 'after-image' from our language, which would be silly, nor even claiming that this could be done.[4] He is simply urging that the experience of having an after-image happens to be a brain process. He says that the sense of 'to be' he intends here is that of strict identity[5]: the experience of having an after-image is identical with a brain process.

When presented with a thesis as paradoxical as this, one's first reaction is to dismiss it as obviously wrong. Then one begins to wonder why anyone should wish to defend it. Smart answers this question: he wishes to defend it because he thinks that anyone denying it must admit that experiences are things or occurrences over and above brain processes, that they are "nomological danglers".[6] In what follows, I shall try to show why one's first reaction to Smart's thesis is entirely justified and that Smart's reason for wishing to defend it is misguided.

1

It might seem at first blush that Smart's thesis could not possibly be correct, on the ground that if an experience of having an after-image were really a brain process, then one ought to be able to say all the same things about both, whereas in fact one cannot. But the thesis cannot so easily be upset. To be sure, nothing seems more plausible than to suppose that if one thing is identical with another, then since there are not two things, but only one, everything sayable about one must also be sayable about the other, and vice versa. But unfortunately this principle is invalid. Suppose the only house Jones owns is at 35 Elm Street. No one would deny that Jones' house is the house at 35 Elm Street: but whereas it is true that to say of Jones' house that it is at 35 Elm Street is to make a significant empirical assertion, it is not true that to say of the house at 35 Elm Street that it is at 35 Elm Street is to make a significant empirical assertion. Again, although the Morning Star is the Evening Star, many things that are true of one are false of the other. For example, in the pre-dawn it may well be true that one can see the Morning Star, but it is surely false that one can see the Evening Star. And if the children stayed up until 9 P.M., it may be true that they stayed up until the Evening Star appeared, but it is false that they stayed up until the Morning Star appeared.[7] Quine, in his essay "Reference and Modality",[8] mentions other difficulties with this principle.

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[1] J. J. C. Smart, "Sensations and Brain Processes", in *The Philosophical Review*, Vol. LXVIII, No. 2 (April 1959), pp. 141-156.

[2] See Gilbert Ryle's "Sensations" in H. D. Lewis (ed.), *Contemporary British Philosophy*, Third Series, pp. 427-43.

[3] Smart, *ibid.*, p. 150.

[4] *Ibid.*, pp. 144-5.

[5] *Ibid.*, p. 145.

[6] *Ibid.*, pp. 142-3.

"Still", it might be argued, "the same *sort* of thing can be said about Jones' house and the house at 35 Elm Street, and about the Morning Star and the Evening Star, whereas with the experience of having an after-image and a brain process, one cannot even say the same sort of thing about them. That is to say, some predicates which are truly applicable to one do not even make sense when applied to the other." But lightning is undoubtedly an electrical discharge, and yet some predicates which are truly applicable to lightning do not make sense when applied to an electrical discharge, and vice versa. Thus a flash of lightning, but not an electrical discharge, can be brilliant, or bright yellow, or have a certain length and shape. (An electrical discharge can follow a certain path, but this alone does not permit saying that it has a length or a shape.) An electrical discharge, but not a flash of lightning, can be heavy or intense. Again, if a man calls a woman an old gossip, what he says is an insult: but whereas what he says can be true or false, an insult cannot be true or false. Hence, although we may suspect Smart's thesis to be wrong just because so many things which can be said of the experience of having an after-image cannot be said of a brain process and vice versa, it cannot legitimately be rejected on this ground alone.

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[7] I borrow these examples from Leonard Linsky. See his "Hesperus and Phosphorus" in *The Philosophical Review,* Vol. LXVIII, No. 4 (October 1959), p. 516.

[8] W. V. Quine, "Reference and Modality" in his *From a Logical Point of View* (Cambridge, Mass., 1953), pp. 139-59.

Nevertheless, Smart's thesis cannot be accepted. Suppose we admit it to be scientifically proven that the experience of having an after-image is simultaneous with the occurrence of a brain process and that whenever, and only when, a brain process occurs, the person has the experience of seeing an after-image. Suppose further that, admitting these two things, we nevertheless wish to maintain that it is not the case that an experience of having an after-image is a brain process. Smart would have to say that we would be in error. But he cannot tell us how we have got it wrong. He can only say that the error consists in asserting that there are two events when in fact there is only one: but this is just to re-assert his thesis, thus denying ours. It is like saying "Your view is wrong because it is wrong". But a mistake of which it can only be said that it *is* a mistake, when nothing can be said as to why it is a mistake or when absolutely nothing can be brought forward to show that it is a mistake, is a very peculiar kind of mistake indeed.

With genuine cases of identity, there seems always to be an additional explanatory statement (or statements) one can make about the two things (i.e. other than the statement itself that they are identical) which is such that if someone grants it (or them), then he must grant that the two things are identical. In the case of Jones' house and the house at 35 Elm Street, the additional statements are that Jones owns the house at 35 Elm Street and that he owns no other house. If someone were to admit these things and yet deny that Jones' house is the house at 35 Elm Street, one would not understand his position: he must either fail to understand some of the key words involved, or be joking, or something. In the case of the lightning and the electrical discharge, the additional statements are that within the volume of space occupied by a flash of lightning, an electrical discharge occurs and that this is the only thing which all such volumes of space have in common. If you grant these two things you must grant that a flash of lightning is an electrical discharge: indeed, to grant them *is* to grant that a flash of lightning is an electrical discharge. But when we look for some additional explanatory statement or statements in the case of an experience of having an after-image and a brain process, none is to be found. At least, I can think of none and Smart offers no suggestion.[9] But if there is no such additional statement, as there seems to be in all genuine cases of identity, then I fail to see how Smart's thesis is intelligible, much less worthy of acceptance.

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[9] To be sure, the additional statement can be made that the one and only thing that persons experiencing after-images have in common is that a certain kind of process occurs in their brain: but this statement supports the identity of people who experience after-images and people in whose brains a certain process occurs, and not the identity of the experiences and the brain processes.

2

The reason Smart wants to maintain that an experience of having an after-image is a brain process is that he thinks that if it is not, then the experience must be something over and above the brain process. He apparently accepts the quite general principle that if you cannot say that one individual is (identical with) another, then you must grant that the latter is something over and above the former. I shall hereafter refer to this as Smart's Principle. Smart suggests that to say that one thing is something over and above another is equivalent to saying the following: if the two things are present in a given situation, then in order to give a full description of the situation, one must mention both things.[10] Hence if it is not the case that an experience of having an after-image is a brain process, then if a person experiences an after-image and a process simultaneously goes on in his brain, then in order to give a full description of what happened to him at that moment, you would have to say that a process occurred in his brain, and, in addition, that he had the experience of having an after-image.

Smart's Principle is a plausible one: it is quite often true. If I passed Jones' house and the house at 100 Elm Street, and Jones' house is not the house at 100 Elm Street, then to give a full description of the houses I passed, one has to say something to the effect that I passed Jones' house and, in addition, the house at 100 Elm Street. But it is also quite often false. Suppose that on page 306 of a certain book there is printed nothing but the word 'dot'. One cannot say that a word is three black marks: yet from this it does not follow that in order to give a full description of what is printed on page 306 one has to say "There are three black marks and, in addition, a word". On the contrary, that statement would be false. It is a full description to say that three black marks are printed there: it is also a full description to say that a word is printed there. Depending on various circumstances, one or the other description will be more nearly satisfactory, but both descriptions are equally full. They are different full descriptions.

Perhaps the situation is the same in the case of the experience of having an after-image and the brain process. Perhaps it is a full description of what is going on in a person at a certain time to say that he is having an after-image, and also a full description to say that a certain kind of process is going on in his brain. If so, then although we cannot say that the experience of having an after-image is a brain process, nevertheless we are not thereby forced to accept the view that experiences are things over and above brain processes. But this suggestion does not seem satisfactory. It seems that both alleged full descriptions would actually be incomplete, and that a full description of what is going on in the person would have to include a reference both to his experience and to his brain process.

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[10] Smart, *ibid.*, p. 142.

But let us consider another counter-example to Smart's Principle which is closer to the case we are interested in: Wittgenstein's duck-rabbit.[11] It is obvious that in general a picture of a rabbit is not a picture of a duck. It might seem, however, that one could say of the duck-rabbit: "This picture of a rabbit is a picture of a duck." But actually one cannot say this. To say anything of the form "This picture of an x is a picture of a y" implies that the x is a y: indeed, it is a devious way of saying that the x is a y. For example, to say that this picture of a man is a picture of a scallawag is to say that the man is (or was) a scallawag. Similarly, to say that this picture of a rabbit is the picture of a duck would be to say that the rabbit is (or was) a duck, which is absurd. We can, of course, say "This picture of a rabbit is *also,* when looked at differently, the picture of a duck", "This line drawing is at once the picture of a rabbit and the picture of a duck", or "Looked at one way, this is a picture of a rabbit, looked at another way, it is a picture of a duck", and so on, but these remarks are quite different from the simple statement that the picture of a rabbit is (identical with) the picture of a duck. But from the fact that in the duck-rabbit the picture of the rabbit is not the picture of the duck, it does not follow, as it would have to follow if Smart's Principle were correct, that the picture of the rabbit is something over and above the picture of the duck: it obviously is not.

It might be that the relationship between an experience of having an after-image and a brain process is analogous to that between the picture of the duck and the picture of the rabbit in the duck-rabbit. That is to say, it might be the case that what can be observed by someone else as a process going on in a person's brain, is experienced by the person himself as the having of an after-image. If I understand them aright, Spinoza, Samuel Alexander, and, more recently, R. J. Hirst[12] have defended precisely this view. (I shall call this the "duck-rabbit" view of consciousness.) If this is the true view, then although one cannot say that the experience of having an after-image is a brain process, it would not follow that the experience is something over and above the brain process.

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[11] *Philosophical Investigations,* p. 194.

[12] R. J. Hirst, *The Problems of Perception* (London, 1959), chapter 7.

A defender of Smart's thesis might make the following rejoinder: "Smart intended his thesis to be just such a view as the one now suggested. After all, his theory may be construed as asserting that the experience of having an after-image is in the nature of an appearance, to the person who has it, of an underlying reality, i.e. the brain process—in much the same way that a jagged yellow flash in the sky, which we call a flash of lightning, is an appearance to an observer of an underlying reality, i.e. the electrical discharge. If the two cases are similar, why cannot Smart say that an experience of having an after-image is a brain process, since one can certainly say that a flash of lightning is an electrical discharge?"

This rejoinder is wrong on several counts. We do sometimes say that certain things have appearances of one sort or another: e.g. "Old Sam has a bovine appearance", "This building has the appearance (or all the appearances) of being a chapel", and so on. But the case of the experience and the brain process is not at all like these ordinary ones. For example, to say that Sam has a bovine appearance is to say that he looks bovine—and not just to the speaker or a few people. But it makes no sense to say that the brain process looks like the experience. Moreover, in this latter case we are alleged to have an appearance which is presented, in principle, to one person alone, and that would be an odd kind of appearance. Moreover, 'appearance' is not the name of a two-termed relation in cases like that of Sam as it purports to be in the case of the experience and the brain process: there is not a something, *x*, which is the appearance of the underlying reality, Sam.

For these reasons, I think it most unlikely that one can legitimately speak of an experience as being the appearance of a brain process. But whether this is so or not, it is very odd that a defender of Smart's thesis should wish to maintain that an experience is the appearance of a brain process, for an appearance is never identical with its corresponding (or "underlying") reality: certainly Sam's bovine appearance is not identical with Sam. What is more, even if an experience were the appearance of a brain process, nothing would follow by analogy from the case of the lightning and the electrical discharge: because a flash of lightning and an electrical discharge do not stand to each other in the relation of appearance to reality, but rather in the relation of one sort of reality to its inner nature. I conclude that the rejoinder fails of its purpose completely.

So far, I have followed Smart in speaking of "full descriptions". However, I think he may have been led into his erroneous identity thesis partly because he mistook a full description for something rather different, namely a full list. It can easily be shown that a full description is different from a full list. Consider the example of page 306 of a certain book, on which just the word 'dot' is printed. A full list of what is printed on page 306 might consist of just the one item: the word 'dot'. But this would not normally be considered a full description of what is printed on page 306: for that, one would have to mention the colour of the ink, the size of the letters, the type of type, and perhaps more besides. I think it is clear from the way Smart speaks of a full description that he really means a full list.

Smart's error does not stop with this confusion. He refers to situations which are such that if one is to have a complete description of them (and he meant, if one is to have a complete list of the elements contained in them), it is necessary to mention two things (call them *x* and *y*). Smart fails to realise that this alleged necessity can take at least these two different forms:

(a) One must mention *x* as one item of the list, and *y* as a separate item of the list.

(b) One must mention both *x* and *y* in the same item of the list.

Smart envisages only possibility (a): and admittedly in such cases *x* is something over and above *y*. But he completely overlooks the possibility of type (b) cases: and in those, *x* need not be something over and above *y*.[13] The duck-rabbit affords an example of type (b). Suppose that on a page of a certain book there is printed a duck-rabbit and nothing else. Then for a full list of the things printed on that page, one would have to mention both the picture of the duck and the picture of the rabbit: for example "There is a figure which is a picture of a duck when looked at in one way, and a picture of a rabbit when looked at in another way". To say only that there is a picture of a duck or to say only that there is a picture of a rabbit would be completely misleading.

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[13] Although it might be. For example, suppose there is in a room a device whose lower half is a stove and whose upper half is a refrigerator. In order to have a complete list of the room's contents, one would have to mention both the stove and the refrigerator, but not as separate items (as if they were two separate pieces of equipment). And yet the refrigerator is something over and above the stove.

But from this it does not follow that the picture of the rabbit is something over and above the picture of the duck, nor that it is a nomological dangler.

Smart apparently accepted the following conditional statement: if in order to have a full list of the things going on in a man, one must mention both the physical processes going on in his body (e.g. his brain processes) and his states of consciousness (e.g. his experience of having an after-image), then things like the experience are entities or occurrences over and above the brain processes.[14] Since he was extremely anxious to deny the consequent of this conditional statement, he felt compelled to deny its antecedent, and he did so by identifying the states of consciousness with the brain processes. Smart thus simply assumed that the necessity of mentioning both the states of consciousness and the brain processes was a necessity of type (a), for it is only if the necessity is of this type that the conditional statement is certainly true: it need not be true if the necessity is of type (b). And in the duck-rabbit view of consciousness, the necessity is of type (b), and there is thus no need whatever to identify states of consciousness with brain processes: indeed, as we have seen, one cannot do so. Hence Smart is mistaken in his assumption that in order to avoid the undesirable dualistic view of mind and body one is forced to identify states of consciousness with brain processes.

I conclude that Smart's thesis that an experience of having an after-image is a brain process cannot be accepted (Part 1), and that his reasons for embracing it are mistaken (Part 2).

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II

By W. D. JOSKE

Professor Smart in his article "Sensations and Brain Processes"[1] proposes an answer to the question "What is it that I am reporting when I report that I have at this moment a roundish blurry-edged after-image which is yellowish towards its edge and is orange towards its centre?" His answer is intended to give us a model for the interpretation of reports of sensations which will enable us to claim that they are really reports, but that they are not reports of things which are irreducibly psychic. He argues that such reports are reports of brain processes.

His reasons for wanting to analyse away psychic things or events are not purely philosophical, but are nonetheless good reasons for hoping that such an analysis will work. In the first place, our description of the universe will be simpler if we can do without psychic entities. In the second place, Professor Smart believes that, if there are psychic entities, the laws which relate them to processes within the human body "would be like nothing so far known in science".[2] They would be ultimate laws which related simple constituents, not to simple constituents, but to configurations consisting of perhaps billions of neurons. These two reasons do not compel us to deny the existence of psychic somethings, but they perhaps make it obligatory for us to push such a theory as Professor Smart's for all it is worth, in the hope that it will withstand all objections. Professor Smart's article in the main consists of eight objections which have been made against the theory and his replies to them.

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[14] Smart, *ibid.*, p. 142.

[1] *Philosophical Review*, LXVIII, No. 2, April 1959.

He realises that his account of the manner in which reports of sensations are reports of brain processes must be subtle and refined in order to overcome the standard objections to such theories. He makes the following five points: 1. He does not claim that "X reports having an after-image" *means* "X reports on his brain processes", but only that reports about after-images *happen to be* reports about brain processes. He does not claim that sensation statements can be translated into brain process statements, but only that "in so far as a sensation statement is a report of something, that something is in fact a brain process".[3] 2. The experience of having an after-image is a brain process, and the "is" is here the "is" of strict identity. 3. The after-image is not a brain process, although the experience of having one is. In a sense there is no such thing as an after-image or a sense datum, but only the experience of having one. 4. When a person says "I see a yellowish orange after-image", he is saying something like "*There is something going on which is like what goes on when* I have my eyes open and there is an orange in front of me". 5. The italicised words are quasi-logical or topic neutral words, and do not specify exactly what it is that is going on. They do not exclude the occurrence of either a brain process or a psychic process, although in fact they are used to report a brain process, and it is up to the neuro-physiologist to describe the specific details of the process.

Professor Smart does not provide us with an example or analogy to illustrate exactly what is the relationship between a report of a sensation and a report of a brain process. His examples of the flash of lightning and the morning star are used negatively and defensively in order to overcome objections to this theory. The following example does, I think, illustrate and fulfil his criteria for the identity of a sensation report with a brain process report.

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[2] *Ibid.*, p. 143.
[3] *Ibid.*, p. 145.

Let us coin the expression "after-radish", and let us agree to use it in such a way that a person who says "I am having an after-radish" is saying that there is something going on which is like what goes on when he has eaten a radish, and that what is going on is in fact a process of the alimentary tract. If Professor Smart's analysis of sensation statements is correct, "I have an after-radish" resembles "I have an after-image" in the following ways: 1. It does not mean that certain things are going on in the alimentary tract, although it is in fact a report of an alimentary tract process. 2. The experience of having an after-radish is an alimentary tract process, and the "is" is here the "is" of strict identity. 3. The after-radish is not an alimentary tract process, although the experience of having one is. In a certain sense there is no such thing as an after-radish. 4. When a person says "I have an after-radish", he is saying something like "*There is something going on which is like what goes on when* I have eaten a radish". 5. The italicised words are quasi-logical or topic neutral, and do not specify exactly what it is that is going on. They do not exclude the occurrence of either an alimentary tract process, or a psychic process, although in fact they are used to report an alimentary tract process. It is up to the gastro-enterologist to describe the specific details of the process. 6. "I have an after-radish" is a report, and not a conventional expressive or sophisticated belch.

What are our grounds for holding that "I have an after-radish" is a report and not a conventional expressive? A report tells us that something is going on, while an expressive merely leads us to believe that something is going on. A report is made on the basis of something going on, while an expressive is expressed because something is going on, but not on the basis of that something. We can only make a report if we know or believe something to be the case, because we can only tell what we know or believe. A report is a claim to knowledge, an ejaculation is just an ejaculation.

"I have an after-radish" is a report because it is made on the basis of something that we know or believe to be the case. What we know need not, because of the topic neutrality of the words in our analysis, be specified, but something must be known, and hence, unless our knowledge is intuitive, there must be means of knowing. We must be able to feel a pain in our stomach, taste a taste in our mouth, hear ourselves belch, smell what is smelled when we eat a radish, or see our cheeks puff out with flatulence as we look in a mirror. If we cannot feel, taste, hear, smell or see we cannot know, and hence we cannot make a report.

How do we know about our brain processes when we report that we are having an after-image? We cannot see, taste, hear, feel or smell what goes on inside our skull. Professor Smart can only argue that we *do* feel (or hear or smell) our brain processes, but that what we feel is so faint and indistinct that it can only be expressed in vague topic neutral words. Even if we concede that it is possible to feel things while sincerely doubting that we feel them, it seems for heuristic reasons impossible that such knowledge can be the basis of our reports of hallucinations and after-images. If the feelings are so vague how can they issue in such precise descriptions of resemblance? Lunatics describe their hallucinations with great specificity, and so do hypnotised persons. It seems much more unlikely that vague feelings should result in specific and detailed descriptions than that simple psychic occurrences should be correlated with complicated physical ones. It might indeed be logically impossible, for what is a vague feeling other than one which can only be described in vague terms?

University of Western Australia.

"MIRROR IMAGES" ARE PHYSICAL OBJECTS: A REPLY TO MR. ARMSTRONG

By M. ARTHADEVA

Mr. D. M. Armstrong[1] has divided his discussion of my article on mirror reflections[2] into three parts. In the first part he presents my solution of the problem from the naïve realist point of view. In the second part he criticises this solution. In the third part he offers an alternative account.

I have no reply to make to the first part of Mr. Armstrong's article for he has presented my point of view very well. I want only to mention, however, that I do not think Professor Austin would agree to call "mirror images" *junior* physical objects and characterise them as only *quasi*-substantial. For my part, I certainly do not wish to make any distinction between the so-called mirror image and physical object on the level of their substantiality, and I do not know of any philosopher who has used or would wish to use the phrase 'junior physical object' of mirror images.

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[1] This *Journal*, May 1959.

[2] This *Journal*, December 1957.

In the second part Mr. Armstrong begins his criticism of my solution. He says, "Now I think we can say that when we look into a mirror what we see is ourselves. But if we do say so, we shall have to realise that we see ourselves *subject* to certain distortions, a distortion of place and the characteristic left-right distortion". Now I agree heartily with most of this: we do see *ourselves* in mirrors. The left-right distortion of which Mr. Armstrong speaks creates no special problem, however, because objects upside-down do also look 'distorted' (or unusual) but nevertheless we are quite content to say that they are the same objects that we see rightside-up. This is something I have already stated in my original article (*vide* the last paragraph) and I cannot understand how Mr. Armstrong missed seeing this.

Mr. Armstrong continues: "But it is possible to show they [the object 'behind' the mirror and the object before it] are two in another way. If I stand two feet in front of the glass, then my mirror-image seems to be two feet behind the glass, . . . *which means that it seems to be four feet away from me,* . . . Now a thing that is four feet from me, to take only the first point, cannot be identical with me". It is here that Mr. Armstrong makes his major mistake: he says that my mirror-image seems to be four feet away from me. But if, as he himself concedes in the beginning of his criticism (quoted in the previous paragraph), what I see in a mirror is *myself,* then the mirror-image is *no distance at all from me.* Mr. Armstrong must accept the consequence of what he says himself!

Mr. Armstrong concludes his criticism thus: "Hence it must be admitted that when I look into a mirror there is at least an *element* of illusion. (. . . a child or dog, on first seeing a mirror, will think there is *another child* or dog *in front of itself* . . .)" Now, in the sense that we can possibly make mistakes *of inference* on the basis of seeing mirror-images, I do not wish to deny that there is an "element of illusion" when we look into mirrors. But this is an entirely different point, and certainly does not provide us with sufficient grounds for considering mirror-images to be different objects from those we see directly.

What kind of reality does Mr. Armstrong intend to give mirror-images? He says we see ourselves in mirrors and yet takes pains to prove that the thing we see in the mirror cannot be identical with ourselves. What alternatives are there except to call mirror-images sense-data, with the consequence that the things before the mirror are also other sense-data, or to call mirror-images physical objects, wholly substantial, adding that we see the same objects twice when we at the same time see objects before the mirror and "behind" it? "The Alternative Account" given by Mr. Armstrong in the third section of his article tries quite hopelessly to mediate between these two points of view.

Mr. Armstrong says: "Now I agree with Mr. Arthadeva that the only things we can see are physical things, hence I draw the conclusion that in such cases [seeing in mirrors] we do not see, but only *think* we see . . ." But, Mr. Armstrong, we are not only *thinking* when we look into mirrors but are doing something else besides. If Mr. Armstrong wishes to say we are *not seeing,* then he should tell us what we are doing when we look into mirrors. Are we experiencing sense-data?

Mr. Armstrong ends his article thus: "To speak harshly, there are no such things as mirror-images, there are only people who believe falsely or are inclined to believe falsely that they are seeing certain things that do not exist behind the surface of pieces of glass. The word 'mirror-image' has a use, but no substance corresponds to the substantive."

Mr. Armstrong must intend to say here that people believe they are seeing when in fact they are not seeing but doing something else. What is the something else that people are doing when they look into mirrors which they mistakenly think is seeing? People are, after all, in this case, not merely believing but believing that they are doing something or other. Mr. Armstrong should supply an answer to the question of what people are doing if they are not perceiving. If he does this, he will find himself moving into waters he doubtlessly considers murderous, the quagmire of sense-data theories.

University of Ceylon.

CRITICAL NOTICES

TIME AND IDEA: THE THEORY OF HISTORY IN GIAMBATTISTA VICO. By A. Robert Caponigri. London, Routledge and Kegan Paul, 1953. 226 pp. 18s. (U.K.).

This is not the book that the English-reading student has long been waiting for to enable him really to get to grips with Vico's thought; it seems to me, indeed, to be much less illuminating than Flint's *Vico* (Blackwood's Philosophical Classics, 1884), slight as the latter's treatment of a number of important issues necessarily is. Professor Caponigri has sought to steer a course between "idealistic" (Crocean) and "realistic" (neo-scholastic?) interpretations, considering that the war of schools "tends to deflect inquiry from its proper object, Vico" (p. 5); but it is impossible, in the case of any forceful thinker, to avoid "interpretation" in the sense of determining which of certain competing strains are "characteristic" and to present a position in which they are all tidily accommodated. Thus, leaving aside the question of development in Vico's thinking, we can say that "modifications of the human mind" could be given a social instead of a personal meaning (or could cover confusion between the two) and that if the social is the dominant sense, the one in terms of which the most coherent position emerges, then it is not correct to say, as Caponigri does on p. 3, that Vico's enterprise "is precisely the recovery of the spirituality and the *subjectivity* of man in history" (my italics).

It is unfortunate that Caponigri, while giving a good many references (though still having substantial passages in which there is no clear indication of whether we are to take ourselves to be confronted with his gloss or with Vico's own formulation), gives very little in the way of detailed quotation or rendering. In addition, there is much in Caponigri's presentation that is obscure and tortuous; the following example, from Chapter II, on "The Natural Law", is by no means atypical. For Vico, he says, the chief problem of theoretical jurisprudence arises from practical jurisprudence, from the fact that a law which is offered as authoritative must have a *universal intention* embracing a class of instances which "in all their particularity" could not have been foreseen or specifically intended in the law. Hence there is a certain conflict between authority and intention, between different "dimensions" of the law. "This universal intention of the law itself cannot be sustained wholly by the law's certitude and authority; on the contrary, it implies, within the intimate structure of the law, the presence of a further element which is, in fact, in opposition to its certitude and authority and which is the immediate vehicle of its intentional universality. Thus there emerges the concept of the 'verum' of the law, its truth, or logical ground and ideal principle. This ideal dimension of the law enters directly into the purview of practical jurisprudence, for it is the genuine mediatorial principle between the law and the instance." Nevertheless, he continues, "the mediation of the law and the instance through the truth, the 'verum' of the law, is dependent upon a prior mediation within the law. For the 'certum' and the 'verum', as dimensions of the law, stand in dialectical relation to each other, indeed, almost as contradictions. For the authority and the certitude of the law, looking wholly to its immediacy, its particularity and concreteness, opposes, at the same time that, in the structure of law, it implies that universality of intention which is the assumption of adjudication in the same manner that universality of intention, the vehicle of the ideality and the truth of the law, to the degree that it transmutes and transcends the mere authority and certitude of the law, would seem to oppose and negate it. Thus there are present within the law the seeds of diremption and absence from itself, unless this opposition between the certitude or authority and the truth or ideality of the law be mediated or closed" (p. 37). All this fuss seems to spring from the fact that syllogism is inference, that the major premise does not imply the minor and does not by itself imply the conclusion. Whatever difficulty this may suggest regarding the distinction between "certum" and "verum" it certainly does not show what is meant by saying that the natural law "is the mediation" between the truth and the certitude of the law. And it does not show, in whatever way the matter may have troubled Vico in his early writings, how it affects his mature view of history.

According to Caponigri (p. 59) "the 'verum' of the situation of law is transformed and completed in the idea of philosophy which pervades and sustains the Vichian science of humanity. In itself, and in its abstract self-identity, philosophy is the science of the 'verum' now understood not merely in the limited and restricted form, in which it appears in the area of law, but in its widest and most inclusive sense as ideality in itself. Its object is that notion of perfection, of totality, pursued through all the reaches and orders of being and rendered present in its most intimate character". (Cf. p. 172: "The heroic or poetic character does not, as a matter of fact, foreshadow the universality attributed to the class-concept by classical logic; it adumbrates rather the quality of universality which characterises the total presence of the subject.") It appears that, in the manner of the concrete universal, "ideality" acquires content and escapes from being merely the character of having a character by its taking on of comprehensiveness or totality and by its absorption of the particular concrete thing, a mind, into the conception of *what comprehends*. Apart from this instability in meaning of "idea", "ideal", and the like, it has still been in no way demonstrated, by quotation or otherwise, that Vico is speaking in any but a loose or extended sense when he identifies "the principles of the common nature of nations" with "modifications of the human mind" or that these modifications are anything but types of social, organised activity, not in any sense "subjective".

But even if it were psychological and not social science that Vico was concerned with, this still leaves the formal question of what can be meant by a "synthesis" of time and idea, which Caponigri takes to be the main problem confronting Vico in setting up the science of humanity. "The idea is no timeless essence; it is the eternal law of the appearance of concrete forms in time. But the speculative problem which adheres is [read *in*] this notion of an eternal law of temporal appearance, stated in its most abstract form, is the relationship of time and idea. The Vichian theory of history, consequently, as a positive theoretical construction, is the determination of the principle, or principles, of the synthesis of time and idea, or, as it may also be stated, of time and form" (pp. 71, 72). It is not apparent what "the modifications of the human mind" could contribute to such a synthesis unless it were a question of the knowing of both terms, nor does it appear how, even on this shaky basis, any synthesis could take place except between things that had been separately grasped (as time and what temporally appears can scarcely be)—except, indeed, between things that were *both* processes, both concrete, each *already* exhibiting the supposed synthesis. The "problem" arises simply from the attempt to treat something as at once temporal and non-temporal.

Similar considerations apply to Caponigri's treatment of the place of "Providence" in Vico's theory. Religion, he says, is for Vico the unshakable basis of human society, so that the "purported discoveries of godless people living in social harmony and innocence" are without foundation. "The philosophical principles of a science of humanity, he maintains, confutes the possibility of such a phenomenon, while every purported establishment of its actuality is traceable to misapprehensions in the reporter. On the contrary, only the sense of an overweening [*sic*] power suffices to set limits to the essential egocentricity of the finite subject and to awaken in it that sense of its own limitations, of its own finitude, and the corresponding sense of the other, which is the ultimate basis of the social consciousness" (p. 95). Thus the operative conception of providence "is a force in history by which the finite subject is directed to the ideal term of its own nature by way of those movements back upon its own finitude by which it would seem to alienate itself from its own reality. Providence is for Vico not only that power which insures the ideal direction of the activity of the human subject of history in the formation of the positive structures of sociality; it is even more the power which converts to that direction a movement whose momentum, so to say, impels in an opposite course . . . Nevertheless, Vico depicts this action of providence as above all else suave and without violence. It is a natural movement, within the movement of history itself and not interjected from without. It insinuates itself into the very movement of the human subject of history so intimately as to become indistinguishable *in actu* from the movements of his own powers. It is only in its effects, and not in the violence of its activity, that providence is in evidence. Where, on principle, there could be only conflict there is peace; where only retrogression, advance and progress; where mere positivity, the glimmer of the ideal. Its channels are the liberty of man, his passions, his will and wilfulness, even his brutality and malice; within each of these, it works its silent magic. As such, Vico is right in asserting that no demonstration of providence is necessary; or, more accurately, in asserting that a science of humanity as he conceived it would itself be the supreme demonstration of providence. For effectively, and as it could be established by the method of such a science, providence is the movement of human history toward its own ideality" (pp. 98, 99).

It is not apparent how something that is impossible "on principle" can nevertheless happen, i.e., what sort of "principle" this could be. The point is again that something is being treated in inconsistent ways—that providence (or similarly, ideality) is taken now as internal and now as external to humanity—and that one or other view has to be dropped. And there is no doubt in which direction the alteration must be made. That which is known "only in its effects" in human life is something which is known only as a phenomenon of human life. "Providence" is simply the fact that things can "go right" *of themselves,* that activities in which we are engaged can be assisted or sustained in ways that were not planned either by ourselves or by anyone else, that there are natural laws of social activity—though their operation will also have to cover the actual planning or "legislating" that occurs, and though it will be possible also for things to *go wrong* in unplanned and unexpected ways. This is quite compatible with the Vichian view that men can know history scientifically because they have made it; the field of study is composed of human activities, but clearly Vico does not think that the agents must (or even can) know all about these—only that their agency gives them the opportunity of having scientific knowledge. There will, then, be no history or social organisation without some deliberation or "reflection", but it will often be mistaken and it will always be secondary to the working of things themselves. This is the historical view of things to which Vico is really pointing—that there are natural *human* as well as non-human processes, and that the existence of "ideality" in the sense of certain human anticipations gives the human no peculiar logical status.

Caponigri, however, attempts to have it both ways and holds, as against Croce, that "the resolution of the inner contradiction of spirit by the immanentisation of providence is not definitive in principle; on the contrary, it [the resolution?] implies the retention of the contradiction, on principle, as the sole evidence of the freedom and hence of the spirituality of spirit. In a word, the unambiguous identification of providence as a principle either of transcendence or of immanentisation does not conform to the characteristics of the operative concept of providence which controls Vico's thought; as a consequence, neither can advance itself as the development of the logical implication of Vico's thought. It is necessary, consequently, to develop these logical implications in a manner consonant with this operative character of the concept, for this last alone, to borrow a term of Vico's, constitutes the 'certum' of the problem, and, in line with this certitude of his thought, to abandon the rigid alternality [*sic*] of these conceptions, transcendence and immanence as inconsonant with that certitude" (p. 106). Thus, Vico's preference for Plato over Aristotle "was determined by the reflection that the latter's metaphysics led to the conception of God as a 'potter, fashioning things outside himself' and therefore could not furnish the principle of moral philosophy", and his polemic against the "jusnaturalists" was occasioned by their deism "whose effect was precisely to exclude that immediacy of presence between the ultimate metaphysical principle and the temporal process of finite being which preoccupied him. As a consequence, the emphasis of his thought lay in the direction of that immediacy; the power he sought to designate by the term providence was before all else an immanent power, in the sense that its activity formed an integral motif in that temporal process" (pp. 106, 107). On the other hand, there is nothing in Vico to suggest that this process exhausts providence or is a self-generation of that principle; rather, to the extent that the "presence of man to himself in history embraces a principle of alternality, of otherness, that principle of alternality must be characterised as transcendent within the process of history itself and with respect to that process conceived as a whole. But providence, as it is a principle of political humanism and a science of humanity, and as it may be demonstrated by that science, is such a principle of alternality and must, therefore, within the terms of the 'New Science' be recognised as transcendent. The terms transcendent and immanent cannot, therefore, be taken as defining a radical and irreducible alternality in Vico's thought. On the contrary, at the risk of paradox it must be asserted that the immanence of the transcendent is the central principle of his historicism and the whole meaning of his doctrine of providence" (p. 107). And, as a final consequence, "it ought to be recognised that the concept of providence as an operative principle in history is not a concept which demands justification from another source. Rather, it constitutes an original and self-sustaining insight into the meaning and the reality of God and of at least one primary dimension of his nature, that of his immanence to man's presence to himself in history as the irreducible alternality of historical process. The concept of God as providence in human history renders the alternality of immanence and transcendence, from the point of view of human history, less rigid than it would at first appear. For the God whose being and activity is recognised in providence is immanent and transcendent in history at once; manifesting, by the naturalness of his operations and their rectifying force, the concrete synthesis of these characters" (p. 108).

There is obviously little argument in all this; but, apart from the scant justification for the therefores and consequently's, the major difficulty is regarding the *form* of this "synthesis". It is not enough merely to say that the relation between immanence and transcendence is not "rigid"; either they are incompatible and there is no synthesis or they are perfectly compatible and there is no problem, no opposition or "alternality" between them—though it would still be interesting to know what meanings "immanence" and "transcendence" would carry for this to be so. But if the rejection of "rigour" means that we can treat them now as opposed and again as not opposed, then this amounts to *failure* to provide an alternative to "immanentism". It is not by any such "synthesis" of incompatibles that Vico contributes to the science of society; it is in concrete and continuing social activities that he finds the driving forces of human history, it is by their place in (the way they "belong to") such activities that he accounts for the various phenomena of human life. In doing so, of course, he finds himself concerned with philosophical problems, problems of method, problems of process and "intention", but it is still by the illumination that he gives to empirical questions that the success of his work on these problems is brought out.

Caponigri connects his discussion of providence with the question of "sacred history". He says (pp. 99, 100) that the function of providence on Vico's view "is the restoration of the movement of history deflected from its ideal term. . . . It restores to integrity that 'broken world', as it has been called, into the presence of which the awakening self-consciousness of man introduces him. That broken world, on Vico's account, is not the result of a diremption between man and nature, but of a constitutive contradiction within man himself. His insight into the internal and constitutive fracture of the human spirit coincides perfectly with the Christian insight and finds its inevitable expression in the Christian image of that plight, the image of fallen man. For this reason, Vico establishes the dualism between sacred and profane history which pervades his theory. Sacred history is the image of the unbroken movement of the human spirit towards its ideal supposition; in this movement it is sustained by a power which, in its pure otherness, mends that fracture. Sacred history is, therefore, normative with respect of the movement of profane or gentile history; it describes the movement which gentile or profane history might traverse were it transmuted into sacred history. But in itself, gentile history is the movement of the human history [read *spirit*?] toward its ideality by way of the excursion through that broken world. In this movement it, too, is sustained by a power which does not, however, appear as pure otherness, but, in some manner, as intimate to that hazardous movement". This power, we learn, is "providence in its operative and dramatic character".

Now, it is not at all apparent what *evidence* there could be of the mending of the fracture, by a power purely other, so as to produce unbroken movement. What is clear is that it could only be a historical process and, more than that, that if "sacred history" is history at all, and not fable or fabrication, it will have to be worked out under the same principles as Vico applies to history in general, by the same critical methods, by the "new science" which combines philosophy and philology. It would, incidentally, be only by an abandonment of these principles that gentile history could be said to have "sacred history" as a norm. But what, in any case, could this mean? It can scarcely be denied that "sacred" peoples have poetry and law and the rest of the major cultural forms in their social life; are these, then, to be regarded as standards or models for the poetry, law, etc., of "profane" peoples? No serious student of history or critic of culture could think so. Many commentators, of course, regard the doctrine of "sacred history" as an inconsistency on Vico's part, an avoidance, conscious or unconscious, of a dangerous question, the raising of which, however, would be forced by actual historical contacts. The general point is still that the "new science" cannot admit the "dualism" in question but must apply its critical principles to "the knowledge of the speech and acts" of *all* peoples—and that Vico's distinctive contribution to thought, whatever his waverings, is a secularist one.

No more weight need be attached to Caponigri's view of the Vichian doctrine of *ricorsi*. He puts forward a number of general arguments designed to show that it *cannot* be a doctrine of recurrence but does not give a detailed examination of the texts on which commentators have based the contention that it *is*. He does admit that, in employing the conception with regard to the "Middle Ages", Vico sees in this period "a returned heroic age", but he goes on to say (pp. 131, 132): "The 'Middle Ages' are a returned barbarism in the strictly methodological sense that to their concrete structures and documents there are applicable, with certain modifications, those canons of interpretation which he has discovered to be valid with respect to the heroic times of the human race. And in the long run this is, perhaps, the most important sense which the term 'ricorsi' is called upon to bear in Vico's thought, for it is the final historiographic value of the term." What this might be taken to mean is that there are certain resemblances between mediaeval society and, e.g., Greek "heroic" society, and that knowledge is advanced by the study of these; but this is hardly adequate to Vico's threefold division of types, or "ages", of society (divine, heroic and human) with a historical order—in accordance with which the reason for calling the "Middle Ages" a heroic age is that it *is* a heroic age.

Thus while, with regard to the controversy on recurrence which has frequently arisen among commentators, it might be worth pointing out that the notion of a *regular* course and "recourse" in the history of any nation does not imply repetition of events in all details, the view (p. 137) that "ricorso" is simply reflection, is a nation's *going back over* its own past so as to induce the contemporaneity of "the full resources of its life", is scarcely worth combating. The main point of controversy has been whether the doctrine of *corsi e ricorsi* implies a progress on the whole (a spiral movement, as it has sometimes been put) or is compatible with just as definite retrogressions as progressions. It is natural that Caponigri, with his conception of history as a movement to "ideality", to man's *total* presence to himself, should reject the latter view; but Croce also can maintain his "progressivism" only by taking it that there is a movement *on the whole,* only by reading into Vico a totalism not in the least involved in his historical findings or in his general conception of the union of philosophy and philology. It is in no way prejudicial to a doctrine of ethical progress (or growth in goodness) to say that, in any given instance, it comes to an end—this does not at all mean that it never happened. And, in the same way, there can be a period of cultural progress (a "classical" period) which comes to an end, and is not to be thought of as "rising again at a higher level" when a new one comes into existence. Classical Westernism appears now to be relapsing into a "barbarism of reflection" just as classical Hellenism did; but, while there have been connections between the two, there is no reason for saying that the one is the other or that they are parts of the history of the same thing—or, again, that the later is a more advanced form of the earlier, or, for that matter, that either of them need be succeeded by something "more advanced".

A curious further observation is made by Caponigri on the "Middle Ages"; after remarking that, by reflection, "man is not so much in the power of truth, as truth is in his power", he goes on to say: "For this reason the 'Middle Ages' may not appear as the clearest example of a returned barbarism; a clearer might be found in the very age in which Vico lived, in the 'Enlightenment' in which reason was raised to such heights and in which it reveals its inner contradictions so clearly. Voltaire, or even more, a Helvetius or a Condillac, is more the type of the barbarian of reflection than the bear-limbed barons of the Anjou dynasty in Naples or the subtle masters of the schools. The barbarism of reflection is the risk involved in the process of 'ricorso', which as an existential undertaking cannot be immune from its peculiar perils" (p. 139). There is, of course, no question of a returned barbarism, let alone a returned heroic age, being a "barbarism of reflection", which is something that occurs in the collapse of "political" or "human" society and is quite distinct from the *primitive* barbarism to which it gives place when the cycle begins again. It might very well be said that the sort of eighteenth century figure Caponigri is referring to is of the type of "the barbarian of reflection", though it is only an age like ours, in which the type is becoming dominant, that would be referred to as a *barbarism* of reflection. But it is only in an *age* of reflection that such phenomena would appear, and

Vico is definitely taking the "Middle Ages" not to be such an age; on the other hand, it can scarcely be doubted that he would take the eighteenth century as still advancing in civilisation (in the characteristics of the "political" age) and therefore not as concurrently degenerating into barbarism. It is open to Caponigri to dissent from Vico's view of the "Middle Ages" as of anything else, but he appears here to be casting aside the important doctrine of the three "ages" without having seriously considered its force—another indication, I would say, of how badly the doctrine of "ideality" fits into Vico's thought.

The same, I think, can be said of Caponigri's account of time. In briefly criticising the conception of a "synthesis" of time and idea, I did not think it necessary to follow out his involved accounts of "time-form" and "time-structure" or to comment in detail on such contentions as that time is the form or order of the "radical and constitutive tendency of man toward the infinite from the basis of his essential finitude" or "is the pattern according to which that finite principle perpetually transcends its determinate actuality in its movement toward a plenitude and absolute which it apprehends as its perfect double, a completely identical and completely other to itself" (p. 77). It seems to me that there is no relevance to Vico's *historical* doctrine of any substantialist view of time or of any suggestion that there could be something in time itself which would help to determine that the temporal order of a certain set of phenomena would be this rather than that. It might still be doubted whether the notion of a "synthesis" of distinct principles is illuminating even for the early works, though they certainly contain a good deal of philosophical confusion. But, when they are treated as merely preparatory and the *Scienza Nuova* (especially what is called the "second" version of it) is allowed to stand out independently, its coherence can be seen to reside in the fact that its philosophy is no less positive than its "philology" or social theory. What is *new* about the "new science" (and what it would not be seriously misleading to call its "immanentist" character) is just that laws can be discovered in the human subject-matter itself, in its own drives or ways of working, instead of being laid down from above or, in any case, coming from outside. It is in departments of social life or types of social activity that the true content of religion, of language, of politics, is to be sought. This, I should contend, is the only way in which Vico's "new science" can be made coherent and intelligible, and it involves the adoption of an empiricist philosophy and the rejection of all normative and totalistic doctrines.

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THE LOGIC OF SCIENTIFIC DISCOVERY. By K. R. Popper. London, Hutchinson, 1959. 480 p. 50s. (U.K.).

Professor K. R. Popper has probably had more influence than any other man in recent decades in the shaping of opinion about scientific method. With the appearance in English of his famous *Logik der Forschung*,[1] this influence will presumably become stronger still. But the book itself will probably not have the impact it would once have had. For in the last forty years or so, among English-speaking philosophers and even non-philosophers, a broad body of opinion about scientific method has slowly been diffused and widely accepted, which, so far as it is at all precise, is substantially identical with Popper's ideas.

1. *Historical remarks*

Such a broad body of opinion cannot of course be distilled into a number of philosophical theses that are clear and unanimously accepted. Yet I think that the body of typically contemporary ideas about scientific method will for most readers be sufficiently identified by the following short list.

We are supposed to have discovered that there is *no* logic of, or in, scientific discovery; that there could not be a "Scientific Method" with capital letters, a method of making conjectures about the unobserved on the basis of the observed which had any *a priori* rationality about it; that the essential thing about science is a *critical attitude* towards theories; that the methods actually employed in natural science are not "inductive" after all, but "hypothetico-deductive"; that such methods do not need or do not admit of philosophical "justification"; that there is no problem at all about induction; that if there were a problem of induction, the theory of probability in the classical or frequency or other sense would be irrelevant to it.

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[1] The text includes, besides a new preface and countless new footnotes, about 150 pages of appendices not in the 1935 original. Most of the material in these new appendices appears for the first time here, and many of the most important things in the book occur in these added pages.

No doubt all these additions help to account for the length of time the present translation has been promised. But we are promised another book, treating of the same problems as the present one and planned originally as an appendix of it, called *"Postscript: After Twenty Years"*.

There is a very great deal that is interesting in the book which is not touched on in this article; most of it because it is not very closely connected with the question of scientific method, but in other cases for lack of space. The latter is the reason for my not discussing, for example, Popper's arguments against identifying the degree to which one statement confirms another with the relative probability of the second, given the first.

The serious misprints I have noticed are as follows:

p. 325, second equation, right-hand side: for "p(c)" read "p(b,c)".
p. 391, eighth line: for "more" read "less".
p. 396, sixteenth line: for "x" read "$\bar{x}$".
p. 400, footnote *2, first equation, denominator: for "z" read "x".
p. 409, footnote 5: for "E(he)" read "E(h,e)".

Such ideas form the intellectual setting of this book. As will emerge, I do not think any of them has much to recommend it. But even on this estimate, Popper's achievements are remarkable, for he has given to such ideas a clear and precise statement, and developed their consequences with a thoroughness and rigour that few philosophers living could hope to rival.

As a preliminary to criticism, it will be helpful to recall that all these ideas have a common root in the history of philosophy, and what that root is. It is, of course, Hume's argument about causation. Hume there showed us that no logical relations, except incompatibility, can hold between statements reporting observations and other empirical statements going beyond the observed. Given the identification, which we share with Hume, of the province of logic or "reason" with *deductive* relations, we seem to have a clear demonstration of the impossibility of inductive *reasoning*; and of the impossibility of a scientific method in the sense of one method of deriving conjectures from observation which logic or "reason" could tell us to prefer to other possible methods.

Although we see no reason to dissent from this argument, we would not dream of permitting ourselves to be troubled about its sceptical conclusions. We remain perfectly convinced that we are under an intellectual obligation to let observation influence our conjectures in just the ways characteristic of natural science, and not otherwise. So we wax ironic about "Scientific Method" but remain, and reasonably, in awe of scientific method.

The idea that what distinguishes the man of science is "the critical attitude" arises as an attempt to mitigate the scepticism of these conclusions. We can see no way in which logic or "reason" can justify our believing, on the basis of observation, *anything* about the unobserved. But we recall that logic or reason can justify *dis*belief, on the basis of observation, in (some) beliefs about the unobserved, because observations can be incompatible with, though they cannot entail, generalisations beyond themselves. Since this seems to us to be the only way in which we can learn from experience, we incline to the idea that all that is distinctive of science is the attitude that will maximise the opportunities for such falsifications to occur, *viz.* the attitude of being critical towards, of trying to falsify, our conjectures.

This is the guiding idea in Popper's philosophy of scientific method.

2. *Popper's account of how observations corroborate theories*

Popper says that a scientific theory is genuinely supported by observation if and only if it survives severe tests, i.e. sincere, ingenious, but unsuccessful, attempts to falsify it.[2]

This is intended as a denial of several things. It is a direct denial of the belief that theories can acquire empirical support by "instantial-confirmation", i.e. by observations of which the most we can say is that they are just such as would be expected if the theory were true. Less obviously, it is also a denial that the relation of supporting or confirming or corroborating,[3] in which (we all intuitively believe) observations can stand to a theory, is to be identified with the probability of the theory relative to the observations; for certain concepts essential to Popper's thesis (such as the "sincerity" or "ingenuity" of the attempts-to-falsify) are, *prima facie* at least, incapable of expression in a formal calculus of confirmation[4] such as Carnap aspires to construct.

Most people will detect, in the view that scientific theories[5] cannot be genuinely supported by instantial-confirmation, an expression of the belief that scientific induction is never simple-enumerative. This belief has become so familiar and widely-accepted, since Bacon wrote, that a writer who accepts it is not called upon to furnish new arguments, or any, for its acceptance. I do not think that Popper offers any arguments for it in the original text; but in some new footnotes[6] there is sketched a new argument for the futility of instantial-confirmation. This argument rests on the fact that nearly everything instantially-confirms nearly every theory. (Since nearly everything is not a raven, and anything which is not a raven or is black is a confirming instance of "All ravens are black".) I have tried twice in print[7] before now to show that this argument creates just as much difficulty for the view that failures-to-falsify support a theory as for the view that instantial-confirmations support a theory, so I will say no more about it, and pass on to examine Popper's own positive view.

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[2] The references here are too numerous to list. See the index under "Corroboration".

[3] Popper now prefers to speak of "corroboration". This is because he thinks that Carnap and others have adopted his coinage, "degree of confirmation" as virtually a synonym for "degree of relative probability"; whereas Popper believes there are decisive objections to the identification of the intuitive idea of empirical support with relative probability. See new footnote *1, p. 251.

[4] Cf. p. 402 footnote 8, and p. 418.

[5] In the sense of empirical, unrestrictedly universal, generalisations. This is the sense in which Popper normally uses the word, and in which I shall always use it except where I give explicit notice to the contrary.

[6] p. 101 footnote *1; p. 258 footnote *4; p. 266 footnote *1.

[7] This *Journal*, Vol. 37 (1959), No. 2, pp. 149-151, and Vol. 38 (1960), No. 1, pp. 51-54, in an exchange with Mr. J. W. N. Watkins, who had made this argument in support of Popper's theory of corroboration explicit. It is gratifying that in his latest reply (Vol. 38, No. 1, pp. 54-58) Mr. Watkins denies (what he at first affirmed) that corroboration requires attempts to falsify a theory, and affirms (what he at first denied) that observations of black ravens confirm "All ravens are black".

First, as to the need for the attempts to falsify a theory to be "sincere". Little reflection is needed to see that this cannot be a *logically* necessary condition of the attempts-to-falsify genuinely supporting the theory. The sincerity of these attempts is, at most, something which, in the absence of more direct evidence, we may take as some indication of the tests' being severe ones. That is, if we ourselves are ignorant of the details of the observations made and the theoretical context, then an assurance that the unsuccessful attempts to falsify the theory have been sincere is for us some slight ground for believing that the tests were severe ones, and that the theory is so much the better supported. What we rely on here is a rough empirical generalisation to the effect that a man who is sincerely trying to falsify a theory is more likely to devise severe tests for it than one trying insincerely to do so. But clearly this creates only a contingent, causal, connection between the theory's being corroborated by the tests, and the attempts to falsify it having been sincere. For let us suppose that we have become as fully acquainted as anyone else with the details of the observations and the theoretical context. Clearly, we are now in a position to judge directly the severity of the tests and the consequent corroboration, if any, accruing to the theory that passes them; and to continue, in these circumstances, to attach any importance to the sincerity or otherwise of the tester would obviously be to introduce a gross psychological irrelevance into our estimates of corroboration.

The case is rather similar with the requirement that the attempts to falsify a theory be "ingenious". We attribute ingenuity to men, and to call some attempts to falsify a theory "ingenious" *might* mean merely that to devise or conceive such tests needed a clever man or unusual intellectual effort from men not so clever. On the other hand, we attribute ingenuity to tests themselves, not in a psychological sense, but as a suggestion that they are or are likely to be *severe* tests. What connects these two senses of the word is of course a well-founded belief that clever men or unusual intellectual efforts from other men are more likely to provide a theory with severe tests than their opposites are. But the connection which this belief creates, between tests' being severe (and so capable of genuinely supporting a theory) and their being ingenious, is a purely contingent, causal one. This can be seen (as with "sincerity") from the fact that a direct acquaintance with those details which allow us to determine for ourselves the severity or otherwise of the test removes any need to take account of *indirect* evidence for the tests' severity, such as the testers' cleverness or intellectual efforts. Thus, to say that observations which genuinely support a theory must be the outcome of *ingenious* attempts to falsify it would be either just an indirect way of saying that such tests must be severe, or the simply false statement that the intellectual psychology of testers of theories is relevant to the estimation of the evidential force of their tests.

Next, about the necessity of *attempting* falsification. One might believe that only the survival of severe tests, and not instantial confirmations, genuinely support a theory, without believing that attempts by us at falsification of the theory are necessary for those tests to be severe. It is a fact, but it is a perfectly contingent one, and has nothing to do with the theory of corroboration, that on the whole severe tests of theories do not occur without our making *efforts,* intellectual and other. We can easily imagine a world in which doing natural science competently was quite effortless. Whatever situations we may contrive to test theories as severely as possible could have been furnished *gratis* by nature. In a world in which such spontaneous severe tests of our theories abounded, there need be little effort or none at falsification, and yet our theories might regularly acquire greater empirical support than they ever do as things are. But even as things are theories sometimes are corroborated by observations made not in the course of attempts at falsification or attempts at anything else, but just by idle observations; by someone not at all concerned with the theory who happened to observe a situation which, if it *had* been found or created in an attempt to falsify the theory, would have been in fact a severe test of it.

It is worth adding that there is no logical connection in the reverse direction either, i.e. that our efforts to falsify a theory are not a sufficient condition, even given their maximum ingenuity and sincerity, of the existence of observations which constitute severe tests of theories. The world could have been such that it was empirically impossible to find or devise such severe-test situations; and in these circumstances no one would dream of attaching evidential importance to the fact that we had *tried* as hard as we could to falsify a theory. Our efforts, that is, count for nothing from the point of view of the theory of corroboration, except in so far as we assume that such efforts are likely to succeed, i.e. likely to issue in severe tests of theories; and if they do succeed, then it is the severity of the tests, and not the efforts of testers to make the testing observations, which supports the theory.

If the existence of severe tests of a theory does not require "attempts" of any kind to have been made, it follows that in particular it does not require the making of "attempts *to falsify*" the theory. But Popper attaches such importance to this requirement that something more must be said about it. It is, of course, a natural consequence of the idea that what is distinctive of science is the critical attitude. It has, too, an obvious, though meagre, empirical basis, in the fact that, other things being equal, a person who is trying to falsify a theory is more likely to find or devise a situation which constitutes a severe test for it than a person who is out to instantially-confirm the theory or a person who has neither intention. Nevertheless, it is plainly a gross psychological irrelevance, as far as the existence of empirical support of a theory is concerned, with what intentions the testing situation was found or devised. To suppose otherwise would be to let differences of intention between two persons who subjected the same theory to the same test affect the question of the support, if any, which passing the test gives to the theory. But this is not all. As a result of insisting that a certain intellectual masochism, a desire to be wrong, is logically essential to the getting of corroboration for theories, Popper seems to persuade himself that the very object of science itself is to be wrong, to succeed in our attempts to falsify our theories! For not only does he always enjoin the selection always of the most improbable of the available theories, but he tells us that the aim of science is that of "ever discovering new, deeper, and more general . . ."—not truths but—"problems"![8]

So far I have written as though severe tests of a theory are necessary, even if certain other things are not, for a theory to acquire genuine support from observation. In one sense of the word "tests" I think this is true. But it is not true in the sense in which Popper and most other "anti-inductivist" philosophers use the word, the sense in which observations constitute a test of a theory only if they are made *with the intention* of getting evidence bearing on *a theory already entertained*. For consider two men, both of whom now defend a certain theory by appealing to certain observations, the theory and the observations being exactly the same in the two cases; but one of whom made the observations after, and because of, having conceived the theory, while the other conceived the theory after, and because of, having made the observations. Here, clearly, we must say, either that the corroboration, if any, of the theory which the observations afford is exactly the same in the two cases, or that the mental history of an investigator necessarily enters into the question of the evidential weight of his observations. If we decline, as surely we should, to say the latter, then we are committed to denying that "tests" in the sense explained above are necessary for observations to corroborate theories; for in this sense only the first man tested the theory.

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[8] p. 281. Even this does not quite match the amazing statement of Dr. J. Agassi (compiler of the index to the present volume, writing in defence of Popper's theory of scientific method in *The British Journal for the Philosophy of Science,* Vol. IX (1959), p. 315) that "the aim of science is to test hypotheses . . . with the hope of refuting them".

The attaching of importance to tests in this sense is part of the wider misconception that we have made a theoretical advance when we say that the methods of science are not inductive but hypothetico-deductive. This is of course a major thesis of this book.[9]

Much depends, of course, on whether this thesis is intended descriptively or prescriptively, as a fact about the natural history of scientists, or as an answer to the question "How does a scientific mind learn from experience?"; and which way it is intended is almost always unclear.[10] But in either form the thesis embodies the belief that it is an essential feature of scientific method as Mill (say) conceived it, that observations should *precede* theory; and this is just false.[11] What Mill's theory of induction, and most others, tried to do is to isolate the circumstances in which observations warrant the acceptance of a theory, and to explain how and to what extent they can do this. To this enquiry it is quite irrelevant whether in fact scientists always or usually or never make their observations before conceiving their theories or *vice versa*. For it is perfectly certain that any observations which an "inductivist" would regard as warranting the induction of a theory *are deducible from* that theory (given only such extra premises as would also be allowed for the hypothetico-deductive testing of the theory); and whether at the time of making these observations an investigator *has actually deduced* them is plainly a psychological question having nothing to do with the question whether and how far the observations support the theory. Our two men of the previous paragraph may help us here, since one proceeded "hypothetico-deductively" and the other "inductively". Now the observations and theory are the same in each man's case; and hence any relations between the observations and theory that are present in the one case are present in the other. But presumably corroboration or empirical support is such a relation.

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[9] See section 3, and the index under "Methods, deductivist view of".

[10] Compare the equivocation in: "What we do—or should do—is hold on to the most improbable of the surviving theories . . ." (p. 419).

[11] It is to this misapprehension that we owe the unabated plague of historical accounts—intended to rebut this supposed "inductivist" tenet—showing how often important theories in the history of science had a psychological genesis which was wildly speculative or even positively irrational.

Hence observations intended to test a theory previously entertained will warrant acceptance of the theory if and only if, and to the extent that, they would have warranted the induction of the theory from the observations if it had not been previously entertained.[12]

Popper and other hypothetico-deductivists have succeeded in making most of us feel that the inhibitions which we got from our empiricist ancestors against approaching nature with preconceived theories are intellectually constricting and stuffy. Perhaps rightly; Mill might perhaps have been an indifferent psychologist of science, though he certainly did not underestimate the practical indispensability of having a hypothesis.[13] But the difference, let alone opposition, between an inductive and a hypothetico-deductive method is, from the point of view of the *philosophy* of science, just nil. The problem about induction, for example (if instead of talking about generalising from experience we prefer to talk about the testing of theories), simply reappears in the form of the problem whether and how a theory's surviving all its past tests warrants any expectations about its performance in future tests.

What we have done so far is to show that none of the things which Popper regards as necessary in order that observations should corroborate theories, is necessary for the existence of severe tests of theories (using the word "tests" now in a sense which is non-committal as to the time-order of observing and theorising). Even so, it could still be true that it is only observations made in severe tests (in this sense) that can corroborate a theory. Everything now depends on what analysis Popper offers of the "severity" of tests.

It seems a strange thing, but he offers none, nor even a hint of one. (The word appears nowhere, that I can discover, in the index.) It might be imagined, since there is no obvious obstacle to an analysis of severity in terms of probabilities, that Popper's measure ("C") of degree of confirmation, which is entirely in terms of probabilities, incorporates such an analysis. But on the contrary, Popper repeatedly insists that the severity of the tests is something that has to be given before we can use his C formula to measure degree of confirmation.[14]

Such absence of any discussion of this central concept can only serve to strengthen the impression—for which, as we have seen, there is ample independent basis—that Popper holds that the existence of corroboration for a theory is logically dependent on the temperament, intentions, and behaviour of scientists.

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[12] As Mill observes, *Logic,* Bk. III, ch. XIV, section 4, fifth paragraph.

[13] See *Logic,* Bk. III, ch. XIV, section 5.

[14] ". . . I must insist that $C(h,e)$ can be interpreted as degree of corroboration [of hypothesis "h" by evidence "e"] only if *e is a report on the severest tests we have been able to design.*" (p. 418. Italics in text.) The same is said on p. 414.

3. *Testing, or trying to falsify, theories = eliminative induction*

Popper does not hold that, of course. There is no reason to believe that his notion of the severity of tests is an unfamiliar or technical one; and the commonsense notion of a severe test can be explained in a quite objective, non-psychological way, as follows. Observations constitute a severe test of a theory if and only if they are (i) such as would be expected if the theory were true and (ii) such as would *not* be expected if the theory were false but are, rather, almost incompatible with its falsity.[15] Instantial-confirmations are not tests of a theory because they satisfy only (i), i.e. because observations which confirm a theory may reasonably be expected to abound even if the theory is false. With only a verbal alteration, these two conditions also give an objective, non-psychological, analysis of a "hard" or rigorous attempt to falsify a theory. For observations could represent the outcome of such an attempt if and only if they are (i) such as would be expected if the theory were true, and (ii) such that contrary observations are nearly certain if the theory is false. (Obviously, for certain observations to be "almost impossible", on a certain assumption, and contrary ones to be "almost certain", are only verbally different.) Since such contrary observations would falsify the theory, condition (ii) can alternatively be expressed as: "such that the theory, if false, is almost certain to be falsified".

Now since we mean by "theories" generalisations of unrestricted universality, observations can be incompatible with the falsity of a theory, as (ii) requires, only if we identify the contradictory of a theory with the disjunction of the contrary theories. In other words, when we speak of observations as being incompatible with the falsity of a theory, what we mean is that they are incompatible with the *alternative* theories, with those theories which were, prior to these observations, possible rival explanations of the phenomena explained by the theory. Hence Popper's thesis, that theories are supported only by surviving severe tests or by our trying hard but unsuccessfully to falsify them, means that theories are supported only by observations which falsify (i.e. are incompatible with) their alternatives.

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[15] "Almost incompatible" is to be understood here, not in a sense involving degrees of incompatibility, but in the sense of "incompatible (absolutely) with most of the possible contraries of the theory". A corresponding sense is intended for "nearly certain" a few lines further on.

But this means that this thesis, with the accompanying adverse judgment on instantial-confirmation, is essentially the old story that scientific induction is never simple-enumerative but always eliminative.

This should have been obvious all along. For when we test or try to falsify a theory, what we are trying to guard against is the possibility that our theory may have excluded something relevant, or included something irrelevant, to the explanation of the phenomena we wish to explain; and the eliminative methods studied by Mill or von Wright are the only means we have of distinguishing, within the complexes we observe, between what is relevant and what is irrelevant to—whatever it may be.

If this general argument fails to carry full conviction we can, I think, satisfy ourselves that testing a theory more or less severely is simply eliminating more or less exhaustively the alternative theories, in the following way.

Suppose there are three pairs of sequences in nature, symbolised as follows: (1): ABCDE-X, FGHIE-X; (2): ABCDE-X, ABGHE-X; (3): ABCDE-X, ABCDE-X.

(If examples are wanted, the most primitive ones are the best. The reasoning from which we conclude that our eyes need to be open in order for us to see, or that a supply of air is necessary for human life, are applications of the method of agreement. It is just by such reasoning that Newton establishes, for example, Corollary I of Prop. VI Theorem VI, Bk. III of *Principia,* or that we establish, by a more refined analysis of the antecedents, that it is not air as such but the oxygen in the air that is necessary for human life; and the incomparably more sophisticated theories which such relatively primitive discoveries make possible are supported by observation in ways not essentially different.)

Now one way in which we can think of the observation of these pairs of sequences is the way in which Mill is usually supposed to have thought of observations. That is, these pairs of sequences if observed are thought of as permitting, with varying degrees of conclusiveness, an induction, according to the method of agreement, of the conclusion that E is the only condition necessary for X. The first pair of observations are ideally conclusive, because they eliminate[16] every theory, other than the E-theory, about the necessary conditions of X.

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[16] Given certain assumptions which must be made before any eliminative method is applicable. Two of these are mentioned in section 4 below. The assumption of determinism, or of the uniformity of nature, takes the form, for the method of agreement, of the assumption that X has *some* (simple, i.e. non-disjunctive) necessary condition.

The second pair is much less conclusive, although it does eliminate some of the alternative theories, leaving the E-theory with a much-reduced number of rivals. The third pair is ideally *in*conclusive, because it fails to eliminate any of the possible theories. (In fact it does not permit the employment of any eliminative method at all, but constitutes only simple-enumerative evidence for the E-theory.)

But we can equally well think of these observations as ones which are sought out or devised in order to test the hypothesis, previously conceived, that E is the only condition necessary for X. We will then find that the severity of the tests, or the "hardness" of the unsuccessful attempts to falsify the hypothesis, which these observations now represent, correspond exactly to and depend on the same things as, the conclusiveness with which the method of agreement can be applied to the observations to yield the E-theory.

Thus, suppose that the first observation (common to the three pairs) merely has the effect of suggesting to us, however irrationally, the E-theory; and that the second observation in each pair is made in order to test or to try to falsify that theory. The presence of E in the second observation in each pair represents, of course, the fact that the attempted falsification fails, that the E-theory survives the test.

Now the first test is an ideally severe one for the E-theory, just because the occurrence of X in such circumstances is incompatible with every alternative theory. Equally, a man who cited the second observation as an attempt by him to falsify the E-theory would be conceded to have tried as hard as possible. For an attempt to falsify is "hard" in the objective sense, as we saw, when if the theory is false it is almost certain to be falsified. But an instance of X such as the second one of the first pair is an observation which is certain to falsify every initially-possible false theory. Given that there is only one antecedent common to both instances, what the E-theory predicts about the antecedents of the second X conflicts with what every alternative theory predicts.

The test represented by the second observation in our second pair is a test, of the E-theory, of more than minimum but much less than maximum severity, just because observation of X in these circumstances is incompatible with most but not all of the theories comprehended under "the falsity of the E-theory". Reported as an attempt to falsify the E-theory, this second observation would evoke the comment that we had not tried very "hard". For here it is far from being the case that every false theory will be found out. A second instance of X which has several antecedents in common with the first is one in which the observed antecedents are certain to be compatible with some initially-possible false theory. Given that the second X has several antecedents in common with the first, what the E-theory predicts about these antecedents will not always conflict with what alternative theories predict. (The set of antecedents ABGHE, for example, though incompatible with some of the initially-possible false theories, is compatible with the A- and B-theories as well as the E-theory.)

The second observation in our third pair clearly represents a "test" of zero-severity for the E-theory, because there is nothing in the observation incompatible with supposing the E-theory to be false. (In fact this is not a test, but mere instantial-confirmation, of the theory.) If a man reported this observation as evidence of his having tried to falsify the E-theory, we would retort that he had not tried at all. For a second instance of X which shares every antecedent with the first is one in which every initially-possible false theory is certain not to be falsified. Given the identity of the antecedents, what the E-theory predicts does not conflict with what any alternative theory predicts.

Thus there is a logical mechanism by which a theory is supported by surviving severe tests or hard attempts to falsify it. It is also that of eliminative induction, the kind of induction which is according to Mill the sole method of generalising scientifically from experience. That mechanism is the elimination of alternatives.

Immediately we perceive this, Popper's methodological injunctions become much more comprehensible. Previously, he seemed to be saying that there is scientific merit in having a certain psychological attitude to theories (*viz.* a sadistic one), and even that observations could not genuinely support a theory unless the making of them was prompted by this attitude. All of which is quite incredible and a warning to all who take for profound truths those commonplaces—harmless and even true at a certain level—of our civilisation about the supreme importance, in intellectual matters, of "the critical attitude". But we now see that, despite their sound, Popper's injunctions to *search* for negative instances, to *try hard* to falsify our theories, are not intended to attach evidential importance to human effort. Some observations are *objectively* more capable of severely testing a given theory than others; some attempts to falsify a theory are "harder" than others, not in a psychological, but in a *logical* sense; and the real import of Popper's injunctions is that we should prefer such observations as evidence.

What makes it difficult to distinguish this import from the psychological language in which it is clothed—and what also explains the practical soundness of Popper's injunctions—is the empirical fact that tests or attempted falsifications which are in the objective sense severe or "hard" are normally psychologically "hard" too. That is, the situations which *could* be used to test a theory severely are normally not easily found but require unusual human effort, including the effort of scepticism, to bring them into being or at least into notice. But such observations, once made, support a theory (if they do) not because we tried hard to falsify the theory but because our efforts did produce observations which would more or less certainly have been different, and thus have falsified the theory, if the theory had been false.

Popper appears not disposed now to deny that the mechanism by which severe tests support a theory that survives them is just that of eliminative induction. No one could accuse him of making the fact clear, but there is in the new part of the book an allusion to it. An imaginary "benevolent critic" accuses him, as I am doing, of turning "inductivist" after all. The critic's ground for this accusation is Popper's insistence that the evidence-statement "*e*", in his measure of confirmation, be reports not of instantial-confirmations but of the severest tests we have been able to design. This, the critic says, "merely consists in an exposition of the so-called 'method of eliminative induction'—an inductive method which was well-known to Bacon, Whewell, and Mill. . . ."[17] Popper does not contest the implied identity of eliminative induction and the testing of theories, even though virtually nothing before this could have prepared the reader for such an admission or even fitted him to understand the critic's somewhat cryptic accusation.

The recognition that testing theories is simply applying eliminative induction to our observations is a result of some importance, for three reasons.

First, it reveals where, in relation to the main historical theories of scientific induction, Popper's theory of method stands. There are two main types of such theories. One is the tradition of Bacon, Mill and Keynes, according to which all scientific induction proceeds by the elimination of alternatives and therefore rests entirely on the variety or lack of it in the observed instances. The other is the Laplacean type of theory, according to which mere "great numbers" of observed instances can warrant a generalisation beyond them. We can now see that Popper is not proposing a third type of theory of scientific induction, and still less a non-inductive theory of scientific method, but is indirectly upholding the first type of theory.

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[17] p. 419.

Second, although Popper's theory is not new, it is a substantially true one. That is, it is a substantially accurate description of the circumstances in which competent investigators in most departments of science regard a theory as well-supported by experience. This is just to say that Popper is right in believing that the methods characteristic of science, as we have known science since the seventeenth century, are eliminative ones. And if we thought that the sole business of the philosophy of scientific method were to describe the typical methods of scientists as history reveals them, this would be the last word. But few philosophers would accept that view, and Popper at any rate expressly rejects such a "naturalistic approach to the theory of method".[18]

Third, recognition of the fact that the mechanism by which testing theories supports them is the mechanism of eliminating alternatives should restore at a stroke much of the reputation of classical writers of the eliminative school, notably Mill. That such a vindication should come from Popper is ironic. The currency he has given to the use of the neologisms "inductivist" and "inductivism" as terms of summary dismissal has been largely responsible for most youngish philosophers of science being totally ignorant of Mill's ideas or at best ignorantly critical of them. Yet a vindication it is. Perhaps we may even dare to hope for a subsidence of the noise about the difference, let alone incompatibility, between an "inductive" and a "hypothetico-deductive" account of scientific method, now that we have it from Popper—as we had it all along from Mill—that "all . . . methods of science . . . proceed by the elimination of inadequate hypotheses".[19]

4. *Why is it "scientific" to test theories?*

Popper equates the rational attitude, that which a man interested in understanding the world ought to adopt, with the critical attitude.[20] The non-psychological residuum of "the critical attitude", we have seen, is the application of eliminative methods to our observations. Hence Popper is committed to identifying the scientific method of arguing from experience with the eliminative methods.

At least since Mill, however, it has been well known that eliminative methods are applicable only to material which is assumed to be deterministic. (The method of agreement, for example, requires the assumption that the property under investigation has *some* necessary condition.) Even supposing determinism to be true, it has been fairly well known at least since Keynes that theories arrived at by eliminative methods are likely to be true only if the materials supplied by observation are not so complex that the number of initially-possible theories is infinite.

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[18] See section 10.

[19] p. 413. I do not mean to suggest, by stressing the agreement between Popper and Mill on this thesis, that I think the thesis is true; for I do not.

[20] Preface, 1958, p. 16.

Consequently one might reasonably expect a writer who, like Popper, thinks that it is rational to adopt eliminative methods in trying to understand the world, to try to show that the conditions which are necessary for eliminative methods to be applicable and likely to lead us towards the true laws of nature, are satisfied; or at least to try to show that the beliefs that the world is deterministic and of finite complexity are well-corroborated.

But Popper's theory of corroboration precludes him from even trying to satisfy such expectations. For both these beliefs are untestable; neither is a falsifiable theory set in a context of falsifiable alternatives. Observations *cannot* be incompatible with either of them or with the falsity of either. Hence, according to Popper, experience could never support a belief in either of them.

He does, accordingly, regard determinism as a metaphysical thesis. But as to the other condition, he positively asserts its falsity; "the number of competing theories . . . remains always infinite".[21] Thus eliminative methods are applicable, and likely to lead to the discovery of the laws of nature, only on conditions (a) whose presence Popper's theory of method could never corroborate and (b) one of which Popper thinks in any case absent.

Hence it is difficult to see how Popper could support his belief that, among the indefinitely numerous possible methods of arguing from experience, the eliminative ones are those which a man interested in understanding the world ought to adopt; that, in other words, it is rational or scientific to test theories. It would be so, it seems, only in a world possessing certain large-scale features which eliminative methods are powerless to give us any reason to believe present.

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[21] p. 419. Such assertions have become commonplace in recent discussions of method, and there are several *trivial* senses in which they are doubtless true. (Almost any theory which is in part mathematical, e.g., must have an infinity of rivals in a trivial sense.) But if the theories genuinely "compete", i.e. yield some conflicting empirical consequences, then the thesis that in every, or even one, context in which we wish to generalise from experience, there are infinitely many possible theories, is patently an unfalsifiable factual proposition; and thus, by Popper's criterion, metaphysical. Even if we deny that an unfalsifiable proposition is necessarily metaphysical, to deny the finite-complexity condition is to hazard so stupendous a generalisation about nature that neither Popper nor anyone else, on *any* theory of corroboration, could have good grounds for the denial.

REVIEWS

Aristotle on Art and Nature. By M. J. Charlesworth. (Auckland University College Bulletin no. 50, Philosophy series no. 2.) Auckland, Auckland University College, 1957. 40 p. Price not given.

It is a good omen for the Humaner Letters that an essay on Aristotle's view of art and nature has been published in New Zealand—an essay which discusses these ideas for their own sake and their inherent philosophical value, not simply out of antiquarian interest. This, it seems to me, is the chief merit of Dr. Charlesworth's *opusculum*: he treats Aristotle's ideas almost as those of a colleague and contemporary. He begins clearly and confidently in his attempt to reconstruct Aristotle's aesthetic, not hesitating even to determine what "he could and should have meant". The drawback of such familiarity, however, is that for Charlesworth it involves dismissing "textual and historical considerations" (p. 4), Werner Jaeger's great work in particular. This to me is doubly incomprehensible—first in that Jaeger's concern throughout his book was interpretation, never the textual and historical as an end in itself; secondly, because in a footnote (p. 39) Charlesworth himself quotes a passage from Jaeger which, if only he had followed it up, would have led him straight to what I believe is the precise and consistent Aristotelian sense of *mimêsis*. He does not make use of it, however, with the result that instead his fifth and final chapter manages to undo what was well begun in his earlier ones.

To show why I disagree with Charlesworth's main theses, I must briefly recall Aristotle's notion of individuation. The well-known Aristotelian adage, that the intellect can know only universals, is subtly qualified in the *Metaphysics*:

"When we come to the concrete thing, whether perceptible or intelligible—of these there is no definition . . . but they are always stated and recognised by means of the universal formula (1036a). Your matter and form and moving cause [are] different from mine, while in their universal definition they are the same (1071a). For knowledge, like the verb 'to know', means two things, of which one is potential and one actual. The potency, being, as matter, universal and indefinite, deals with the universal and indefinite; but the actuality, being definite, deals with a definite object—being a 'this', it deals with a 'this'." (1087a) (tr. Ross).

Charlesworth, in his chapter on "the metaphysics of making", seems at first to be aware of all this, but then proceeds to deny it, looking at it through Thomas Aquinas' eyes rather than Aristotle's.[1] He denies that for Aristotle poetry "expresses the essence", "is essentially ideal". But this should have followed from Charlesworth's own statement, that art falls under intellectual judgment—for what should the intellect judge if not universals or essences? In this at least Plato and Aristotle are agreed, nor are Coleridge and M. Gilson merely "imagining that they are being true to Aristotle" (p. 20). Earlier, Charlesworth quotes the dictum, "The artist makes a 'such' out of a 'this'." But this does not mean, as he thinks, that the artist makes *such* a bed out of *this* piece of wood. On the contrary, he makes this particular Chippendale bed out of *such* wood—as oak, or ebony. The meaning of the quotation is that the artist gives his artefact the essential, and that this manifests the universal. Picasso's rooster allows us to see, not, as Charlesworth says, the "specificity" of the rooster, but the universality. While it shows what is specifically this rooster, it can show us what is essentially a rooster. Thus at least Aristotle and Coleridge. Poetry is "in accordance with the probable or the necessary" because it is in accordance with the universal. This is not just "speaking loosely" (p. 21). It is at the very heart of the theory of *mimêsis.*

The difference between Plato and Aristotle on this point is not that Aristotle confines himself to the particular. It is that while for Plato art should attain the essential and universal without intermediary, for Aristotle art can attain it only mediately, only by way of the particular. It is in this that art is like nature: as in nature, the universal, the essence, or the *telos,* is realised only in and through individual things. It is their substance.

Thus, though Charlesworth is moving in the right direction, there is something unsatisfactory about his fourth chapter, where he explains that "art imitates (is analogically the same as) nature (considered as a cause of coming-to-be or of some matter being caused to assume some form)". The word "some" leaves too much in the air. The word "matter" hesitates between matter in its strict sense (*hylê*) and a (formed) material that the artist uses, and the phrase "is analogically the same as" seems an anticlimax.

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[1] The question of individuation is fully clarified in the *De Anima* (which Charlesworth regards as a "minor treatise"), from which it can be seen that the notion of a positive individuation through matter is wholly un-Aristotelian. It was perpetrated, as far as I know, by Thomas Aquinas, in order to combat the Aristotelian-Averroist theory of the possible intellect. Aquinas brings forward such "arguments" as: "If it is matter that makes me individual, how can my mind be distinct from that of Socrates in the possible intellect? How could I ever say a thing is true and Socrates that the same thing is false?"—arguments resting on an assumption which Aristotle would certainly have rejected.

Is *mimêsis* then simply "being-analogically-the-same"? Would this not make the whole theory something of a platitude?

Charlesworth seems to realise this danger, and therefore has a surprise for us. After establishing this sense of *mimêsis* and exorcising the common one of "direct representation" as "far from Aristotle's true intention" on pp. 24-5, he states on p. 26 that Aristotle also uses *mimêsis* in this "bad sense", and on p. 27 that "this is the sense in which the word is used, for the most part, in the *Poetics*". One can hardly believe one's eyes.

Charlesworth has three passages which he claims in support of his revision; let us look at them. The first is from *De Partibus Animalium,* 645a (Oxford tr.): "It would be strange if mimic representations of them [animals] were attractive, because they disclose the mimetic skill of the painter and the sculptor, and the original realities were not more interesting." Not a word, however, about "*mimic* representations" or "*mimetic* skill" is in the Greek. It is only an inadequate translation that has led Charlesworth into assuming that the original was concerned with *mimêsis.*

Next there are two statements that what imitators imitate are actions. (To translate *mimountai* as "represent" would beg the question.) The words (*prattontas, mimêsis praxeôs*) are precisely what in the Latin tradition was conveyed by *operatio*: "the *operation* of art imitates the *operation* of nature", as Charlesworth quite correctly says on p. 24 to support the philosophical sense of *mimêsis.* Two pages later he sees it as an instance of *mimêsis* meaning "direct representation"! The final instance (p. 28) is a passage where Aristotle is said to progress from the one sense of "imitation" to the other. Yet the word *mimêsis* occurs only once in the passage which purports to show this!

There are, then, no two senses of *mimêsis* in Aristotle. Nothing he ever said could lead us to think so. And it is just as well there are not, or we would have in Aristotle, as in Charlesworth, first a metaphysical platitude, "art is creative in more or less the same way as nature is", second the theory of "truth to nature" exemplified in photographic painting and programme music. "Form" and "plot" become synonymous, everything must "tell a story". This is what Charlesworth tries to make palatable in his closing pages. But he did not need to; and as he has "reconstructed" Aristotle's argument, I hope he will permit me now to try briefly to reconstruct his reconstruction.

The parallel between art and nature, as Jaeger pointed out, is grounded in Aristotle's teleology. This is clearest in the *Metaphysics,* in a sentence where Charlesworth did not suspect it, though he quotes it: "Art is a principle of movement in something other than the thing moved; nature is a principle in the thing itself." But art tries to be as like nature in this as possible—it tries to make the formal cause of the work (all that the artist tries to convey) and the final cause (the effect he intends) completely immanent in the work itself, to make the work as organic a whole as possible. As in nature, everything about the work must be there for the sake and furtherance of the whole, and to achieve this the artist must enter his materials and his symbols as completely as nature enters her creations. As the *De Anima* should have shown Charlesworth, in an important sense Nature for Aristotle *is* more than "simply the ensemble of individual natures" (p. 23)—the artist imitates *natura naturans,* "great creating Nature", nature as she works; not *natura naturata,* nature as she looks. The work will give the delight intrinsic to it "like a living being that is one and whole" (*Poetics,* 1459a). The artist imitates nature's *operatio* by entering his work so fully that almost like one beside himself he assumes the moods and passions that he wants to recreate (1455a). In so far as he makes the teleology inhere in his work, the work can exist in its own right (become "pure act", in Aristotelian terms), and can become a symbol for him. The two aspects of this process are completely correlative—and this, I think, is the element of truth behind paradoxes of a more Romantic kind, such as that the most personal art is the most universal, that the greatest art has the greatest simplicity, that in so far as a symbol is fully realised it is inexhaustible, and so on. It also accounts for Aristotle's own paradox in the *Politics,* that music is the most imitative of all the arts: its *telos* is the most fully intrinsic one of all, and is achieved most directly; not even words or shapes or signs can come between.

For Aristotle art also imitates nature in its movement. Each part of a narrative or dramatic work must follow from what precedes "in accordance with the probable or the necessary", as in nature all causality is reducible to the (necessary) formal cause. Art is like nature too "in the balance or reconcilement of opposite or discordant qualities: of sameness, with difference; of the general with the concrete; the individual with the representative". The words are Coleridge's, but Aristotelian in spirit.

The conception of *mimêsis* as an active principle throws a certain light on all the key concepts in the *Poetics.* What is *peripeteia,* for instance, if not an imitation of nature's *praxis*—to proceed from an ordered state, through a crisis, back to reconciliation and order again? *Anagnôrisis,* similarly, is first a change of this sort: from joy to sorrow, sorrow to joy, ignorance to knowledge; second, in a wider sense, a paradigm of nature's re-ordering: the recognition that what is tragic under a particular aspect is no longer tragic but acceptable under a more universal one. Finally, the concept of *katharsis* seems to me the equivalent in the audience of the artist's own act of *mimêsis*. One can see it thus even without taking sides in the debate about its meaning, whether it is a metaphor of ritual purification or of medical purging. Whichever it is, we are left with a twofold process: by going to a tragedy and submitting to its "treatment", by "entering the sanatorium", as Eliot would put it, one emerges blessed with health; or else, by partaking in the sacrificial rite one wins the sacramental grace. The process has the same structure in both situations (as the fact that Eliot could poetically identify them might suggest), and is the precise counterpart of the artist's first undergoing the moods and passions involved in his work, then winning thereby the detached and finished product. What Charlesworth calls Aristotle's "producer aesthetics" does not, as he thinks, exclude a "consumer aesthetics"—on the contrary, a perfectly complementary one can be deduced from it "in accordance with the probable or the necessary".

These are some brief indications of ways in which Aristotle's concept of *mimêsis* might be truthfully developed. They are not, for the most part, new—yet despite the immense exegetic traditions that have surrounded the *Poetics*, the work of interpretation and discussion is still far from done.

E. P. M. DRONKE.

SOVEREIGNTY: AN INQUIRY INTO THE POLITICAL GOOD. By Bertrand de Jouvenel. Translated by J. F. Huntington. Cambridge University Press, 1957. 320 p. 27s. 6d. (U.K.).

This is a disappointing book to get from the author of such a well-worked-out essay as *Power*, and various of the author's own comments (in the Preface and on page 169) support the feeling that it went too soon to press. The four parts into which it is divided deal with authority, the political good, the sovereign, and liberty, but the connexions between these themes are not very firmly established. The section on authority, which draws heavily on the work of Georges Dumézil, is concerned to emphasize the distinction between the leader who initiates action and the leader who maintains an activity in society, between the *dux* and the *rex*. The third section, on the sovereign, is an essay on the growth of sovereignty and of the idea of absolute sovereignty, and will be of especial interest to historians.

It is in the parts devoted to the political or common good and to liberty that we find de Jouvenel's political philosophy, and much of it is retracing old ground. He devotes pages, for example, to arriving at the view that the function of the sovereign (that is, what the sovereign *ought* to do) is to conserve the stability of society while allowing its individual members the responsibility of free enterprise and experiment, a familiar enough liberalism, and one not unlike Bosanquet's or Green's conception of the hindrance of hindrances, although not as clearly worked out. His discussion of the common good, which he identifies with social stability or equilibrium, is not (I think) up to the prevailing philosophical standards of our day, and contrasts markedly with his discussion of justice, which is quite excellent.

Although he does not so express himself, he successfully shows that 'just' and 'justice' are incomplete or relative terms, and he does so by arguing that in almost all uses these terms presume an 'equality of proportions'. We do not, that is to say, think merely that it is just that A should get B, but rather that A should get B *in virtue of his having C,* and disputes about justice are in fact disputes about C, or what he calls the 'relevant criterion'. Even at this point his argument is too loose in texture to secure the conclusion he draws, that 'it is impossible to establish a just social order', but he does sketch the outlines of such an argument and it is one worth further development.

His remarks on liberty do little, on the contrary, to advance that subject. He discusses the two doctrines which he calls 'liberty as power' (that is, I am free when I am able to do as I will) and 'liberty as dignity' (that is, I am free when what I am obliged to do coincides with what I think I ought to do) in a way that sheds no new light on them, and obscures some of the perceptions we already have; and, while he rejects both doctrines, he does almost nothing to put anything in their place. Here and there we find hints that 'what is needed' is a return to something like mediaevalism, but even this is not stated openly and convincingly. And yet, throughout his discussion here (as in the rest of the book), there are scattered casual remarks and observations which, if they settle nothing, at least set the mind on new or half-forgotten paths.

R. E. DOWLING.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

ADKINS, Arthur W. H. Merit and responsibility; a study in Greek values. Oxford, Clarendon Press, 1960. xiv, 380 p. 68s. (Australian).

Traces the development of the concept of moral responsibility from Homer to Aristotle, and discusses the reasons for the relative unimportance of this concept for the Greeks.

ANDERSON, Nels. The urban community; a world perspective. (International Library of Sociology and Social Reconstruction.) London, Routledge & Kegan Paul, 1960. xvi, 500 p. 59s. 3d. (Australian).

DAS, A. C. A modern incarnation of God; a commentary on the life and teaching of Sri Ramakrishna. Calcutta, General Printers & Publishers Ltd., 1958. xiv, 310 p. R15.

DAS, A. C. Sri Aurobindo and some modern problems. Calcutta, General Printers & Publishers Ltd., 1958. x, 192 p. R10.

EWING, A. C. Second thoughts in moral philosophy. London, Routledge & Kegan Paul, 1959. viii, 190 p. 21s. (U.K.).

Anxious to make all reasonable concessions to "the new subjectivists", Dr. Ewing restates and revises his position. "Non-natural qualities" are abandoned, but he remains predominantly objectivist.

GARNETT, A. Campbell. Ethics; a critical introduction. New York, Ronald Press, 1960. viii, 526 p. $5.50.

An historical treatment. After discussing hedonism, intuitionism, self-realisation, and emotive and prescriptive theories, concludes that moral rules are "natural requirements for the wholesome development of personality, norms of spiritual well-being or 'the health of the soul' ". A quarter of the book consists in readings from miscellaneous sources, including Pius XI and Maude Royden.

GELLNER, Ernest. Words and things. London, Gollancz, 1959. 270 p. 25s. (U.K.).

A forthright attack, which has already caused some controversy, on modern linguistic philosophy.

GIBSON, Quentin. The logic of social enquiry. (International Library of Sociology and Social Reconstruction.) London, Routledge & Kegan Paul, 1960. x, 214 p. 24s. (U.K.).

"In Part One scientific procedure in social enquiries will be defended against criticism. At the same time it will be admitted that there are logical peculiarities which the subject-matter of the social enquiries forces upon us. These will be examined in Part Two." Introduction.

HALMOS, Paul R. Naive set theory. Princeton, Van Nostrand, 1960. viii, 104 p. $3.50.

"The purpose of the book is to tell the beginning student of advanced Mathematics the basic set-theoretic facts of life, and to do so with the minimum of philosophical discourse and logical formalism." Preface.

HARRÉ, R. An introduction to the logic of the sciences. London, Macmillan, 1960. viii, 180 p. 34s. 9d. (Australian).

"Intended to provide a short and systematic account of the workings of the sciences for those students who are taking Logic or History of Science as subsidiary subjects." Preface.

HARRIS, Marjorie S. Francisco Romero on problems of philosophy. New York, Philosophical Library, 1960. x, 114 p. $3.75.

Exposition of the views of an Argentinian philosopher. "Absolute transcendence is the primary characteristic of spirit, which is the highest plane of reality."

HARTLAND-SWANN, John. An analysis of morals. London, Allen & Unwin, 1960. 208 p. 25s. (U.K.).

Readable introductory book. Analyses the logic of everyday moral arguments, the difference between moral and non-moral decisions, and so on. Uncompromisingly subjectivist.

HARTMAN, Robert S. La estructura del valor; fundamentos de la axiologia cientifica. Mexico, Fondo de Cultura Economica, 1959. 334 p. Price not given.

HENDEL, Charles W., ed. John Dewey and the experimental spirit in philosophy. New York, Liberal Arts Press, 1959. viii, 118 p. $3.00.

Four lectures given at Yale to commemorate the centenary of Dewey's birth. The authors are Charles Hendel, Nathaniel Lawrence, Richard Bernstein and John E. Smith.

JESSUP, Bertram. Philosophy in Shakespeare. Eugene, University of Oregon, 1959. 24 p. $1 (paper covers).

A lecture given at the University of Oregon.

LEVITT, Morton. Freud and Dewey on the nature of man. New York, Philosophical Library, 1960. x, 180 p. $3.75.

Compares and contrasts Freud and Dewey.

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